

THE PSYCHOLOGICAL REVIEW

PROLEGOMENA TO PSYCHOLOGY¹

BY WILLIAM McDUGALL,

Harvard University

Psychology is, or aspires to become, a science, a systematically organized and growing body of knowledge. Entering upon the study of this science, you will properly and naturally expect to be told what is the class of things, objects, or processes with which the science is concerned, what kind of knowledge, what sort of increase of understanding, we may properly expect to gain from the study of it.

The most satisfying answer is that it should help us to a better understanding of human nature. The aim of psychology is to render our knowledge of human nature more exact and more systematic, in order that we may control ourselves more wisely and influence our fellow-men more effectively. There is probably no psychologist who would find serious fault with this statement. As a definition of the province of the science it falls short in two respects. Such a definition should indicate all that falls within the province, and should exclude everything that does not. In both these respects the statement falls short of perfection. For one well established branch of psychology studies animals, and is properly called the study of animal behaviour. And, on the other hand, we have anthropology, a study which by its very title claims to be, and in practice is, the science of mankind; but which, as generally understood, includes much that falls outside the province of psychology. We should not attach great importance to these imperfections; for similar

¹ The opening chapter of a forthcoming volume entitled — "A General Introduction to Psychology."

difficulties arise when we attempt to define concisely any science or branch of science. The fields of the various sciences overlap. It is inevitable that they should do so; for, if there are sharp divisions in nature, we do not know exactly where to find them and therefore cannot draw any precise boundaries between the sciences. And such overlapping of the sciences is really advantageous; for it brings the workers in the several sciences into touch and co-operation one with another.

The psychologist may and should study animal behaviour; in doing so, he enters the field of the zoologist, needs his help and may hope to render some help in return. But he studies animals for the sake of the light which such study may throw upon his own problems, the problems of human nature. The relation of psychology to zoology is not unlike that of zoology to geology. The zoologist or biologist needs some knowledge of geology, and is able to make returns to the geologist for the help he gets from him; and there is a large field of overlap, the science of fossil remains or palaeontology, which is dependent upon and supplementary to both these more fundamental sciences. In a very similar way, the science of animal behaviour stands between zoology and psychology, as a field of overlap which is dependent upon both and in which they may come into helpful relations.

The other weakness of our definition of psychology, namely that it may seem to claim too much and so encroach upon the field of anthropology, is no more serious. Anthropology, broadly conceived, concerns itself with a man as an animal species. Within this wide science are several more special anthropological sciences, none of which can be sharply marked off from the more inclusive science or from one another; such are ethnology and human morphology and physiology. Psychology is a member of this group of anthropological sciences; and it may be distinguished from the wider science of anthropology by saying that it is concerned, not with man as one animal species among others, but with man in his distinctively human aspect. Now every one knows that man is chiefly distinguished from the animals by his mental

powers. Why not then be content (as some of the earlier writers were) to define psychology as the science of mind, or of the human mind, or of mind as manifested in the human species? There are several objections to such a definition. First, 'mind' is a vague word, itself in need of definition. The meaning of the word can not be defined by pointing to one mind after another and saying 'This and this and this is what I mean by 'a mind.''' Although the words 'mind' and 'mental' are in common usage, we can only gradually build up and clarify our conception of 'mind' or of 'a mind' by prolonged study. Secondly, there are other sciences of mind than psychology: such are logic, and metaphysic, and epistemology and theology, all of which claim or aim to tell us about mind or minds.

Those who have been content to define the science as the science of mind have for the most part conceived of human nature as a combination of two very unlike things or principles, mind and body. But this is an assumption the validity of which is highly disputable; it has been not only disputed, but also confidently rejected, by a great number of philosophers, as well as by many of the leaders of modern science. And, even if we regard the assumption as well founded, we have to confess that it is impossible to distinguish clearly and confidently between body and mind, between the working of the body and the manifestations of mind in or through the body.

The difficulty is that each of us has no direct or immediate acquaintance with minds other than his own. Each one of us experiences pain and pleasure and various emotions, thinks and strives, remembers and expects and resolves. And it is generally agreed that all such experiences are manifestations of his mind or mental capacities. By reflexion upon such experiences a man may form some notion of what his mind does and can do. And, by comparing notes with other men, he learns that they have similar experiences upon similar occasions, and infers that they have minds not unlike his own. Such observation of the varieties of one's own experience is called *introspection*. Every intelligent person

can and does to some extent notice and remember his experiences; and there are very few who do not sometimes describe their experiences in words, reflect upon them and exchange notes with their fellows upon their experiences. When such introspection, reflexion, and interchange of descriptions of experiences and of reflexions upon them is conducted systematically, the process constitutes one of the great methods of psychology. It has been for a long time a well recognized method; and it has in fact often been declared to be the sole practical method of psychological study, the only legitimate and effective method of obtaining knowledge of the mind. During the last half-century this method of study has been greatly refined by the use of systematic experiment; that is to say, the person who wishes to notice and describe his experiences of any particular kind, instead of waiting until in the natural course of events such an experience occurs, deliberately seeks or arranges conditions under which some such experience is likely to occur, expects it and notices it and describes it as carefully as possible. By the aid of a laboratory, all sorts of ingenious apparatus, and skilled assistants, much may be done to refine introspection and to record its results more accurately; and such work is a large part, though by no means the whole, of what is called 'experimental psychology.' Experimental introspection has obvious limitations. Many of the most vital and interesting experiences, such as grief or joy or fear or moral struggle, cannot be induced at will, except, perhaps, in very slight degrees. And, under the most favourable conditions, introspection of our more vivid and vital experiences is difficult, because we are apt to be primarily interested in the events of the outer world in which we are taking part, if only as observers. Then again the very act of introspection does to some extent modify the experience we wish to observe and describe; so that in introspecting we partially defeat our own purpose.

Another great difficulty meets us when we come to exchange notes with others upon our introspections; namely, the language in which we describe our experiences to one

another is always sadly inadequate and imperfect. It is not true, as has sometimes been said, that language was evolved purely for the description of material things and events; it seems more nearly true to say that language was in the first place essentially a means for communicating and describing our experiences, and that, throughout its development, this has been a very important function of language. Nevertheless, in respect of this function, language, in spite of all the efforts of literary men and of psychologists to render it more precise and effective, remains a very inadequate instrument of communication. For the description and discussion of things and events of the material world language has become a very efficient instrument; because we all have, or may have, the same kind of acquaintance with those things and events; and the efficiency of language for this purpose affords a strong guarantee of the essential similarity of such knowledge and acquaintance obtained by men in general. But, in respect of the description of our experiences, language can never attain the same efficiency; just because each man has one kind of acquaintance with his own experience, namely, a direct acquaintance, and another, a very indirect kind of acquaintance only, with the experiences of other men.¹ Yet, here again, the fact that we do succeed by the aid of language in making one another understand in some degree our descriptions of our experiences shows that one man's experiences are not wholly unlike another's, but rather have much in common. In many cases of the description of experience, language is but little less efficient than for the description of objective fact. If I say 'I saw the moon rise over the hill just now,' you understand what I mean almost as fully and as surely as when I say 'The moon rose over the hill just now.' Yet in the former case my words describe a fact of my experience with which you can have only indirect acquaintance through

¹ This statement is sometimes disputed. There is some striking evidence in support of the view that one man, A, may sometimes become aware of the thoughts or feelings of another, B, or otherwise be influenced by them, in some more direct fashion than the usual round-about processes of bodily or verbal expression by B and the perception of those expressions by A by aid of his senses. Such communication by unknown means is generally called 'telepathic', but its reality is not regarded as fully established.

my description; while in the second case the words describe an objective event which you may be acquainted with in the same way as myself and may objectively verify in other ways. Again, I say 'I am truly sorry'; and (if I am speaking truly) I describe a fact with which you can have no direct acquaintance, such as I have; yet you know very well what I mean and you adjust your conduct accordingly. And if, instead of 'sorry,' I had used any one of some hundreds of words, and had said 'I am (or I feel) weary or angry, or anxious, or afraid, etc.,' you might have felt the same confidence that you took my meaning. This confidence is justified by the success with which we use such language to influence one another.

The introspective method has, then, peculiar difficulties and limitations; yet, in spite of these, it is possible for it to achieve a generalized description of types of experience. It could and did achieve a certain stage of psychological science, namely, the descriptive classificatory stage, which is but the first stage of the development of a science. But even this could be achieved only by taking note of the conditions under which we enjoy the experiences that we more or less successfully describe in words. For it is largely by noting and pointing out to one another such conditions that we attain to a common use and understanding of the words by aid of which we describe our experiences. We note for example that 'hot' is the right word for the description of our experience when we closely approach the fire, 'pain' when any part of the body is injured, 'weary' when we have worked long and hard, 'pleased' when we attain what we seek, and so on. Some of these conditions are facts of the outer world, some are facts of experience. By noting systematically such occasions or conditions of various types of experience, it is possible to establish a certain number of empirical rules which raise to the explanatory stage the purely descriptive psychology attainable by introspection alone. This has been done with good effect.

A third great type of observation enables us to carry yet further our understanding of our experience, and at the same time raises another group of problems. This is the observation

of conduct or behaviour, both our own and that of other persons. By the term conduct or behaviour of any person we denote every movement, and every other observable change of bodily condition, which seems to express his experience. We know from observation of our own behaviour that movements of the limbs, the face, the throat and chest, and other bodily changes, such as sweating and shedding of tears, are apt to accompany or follow upon experiences of certain types, in a more or less regular and orderly fashion. And observation of other persons, combined with their statements about their accompanying experiences, enables us to formulate a number of general rules, stating the correlation or conjunction of types of experience with types of bodily expression or behaviour. Common observation has embodied in common speech a number of such empirical rules of correlation between modes of experience and modes of behaviour; and, aided by these and by his own observations, every man, without special study, acquires some skill in interpreting modes of behaviour as expressions of experiences.

These three kinds of observation—namely, (1) introspection, or the noticing of one's own experiences (2) observation of the conditions or occasions of experiences (3) observation of the expressions of experiences,—are practised by all men with some degree of success; and common speech embodies many general propositions based upon these three kinds of observation. Such propositions constitute the popular psychology which we inevitably acquire in learning the use of language. We inevitably make use of these forms of speech and accept more or less uncritically the propositions expressed or implied by them, until we begin to study psychology scientifically. Most of these implications of common speech are more or less true; for they embody the wisdom of the ages. But most of the words used in such popular psychology are so vague and ill-defined, that it is very difficult to formulate any unambiguous propositions or to convey any clear meaning in terms of such popular psychology. The literary art largely consists in so combining the words and phrases of popular psychology as to convey such meaning more effectively than

the common speech of the common man can do. The cultivation of literature or the art of letters has thus greatly refined the psychology of common speech; and, when we speak of a cultivated man, we generally mean one who has learnt to appreciate and to make use of this more subtle and effective literary psychology. Persons whose education has been mainly literary, and who have learnt to appreciate highly and to use with some skill this more subtle literary psychology, are very apt to take the view that it is the only form of psychology of any value, and that the attempt to deal scientifically with the facts of experience, or to make a scientific psychology, is mistaken and fruitless. But this anti-scientific view is ill-founded; there need be and should be no antagonism between literary and scientific psychology. From this point onward I shall use the terms 'psychology' and 'psychologist' to denote the scientific varieties; but, in doing so, I shall imply no disrespect for the achievements in this sphere of poets and biographers and writers of romance. The wise psychologist will regard literature as a vast store-house of information about human experience, and will not neglect to draw from it what he can.

For long ages the literary and the scientific treatment of human nature and human experience were but little differentiated; but as, under the stimulus and example of the other natural sciences, psychology began to be studied more systematically for its own sake, it drew further away from the popular and literary traditions, and a spirit of antagonism showed itself. This tendency was perhaps inevitable; for the psychologists, in attempting to specialize the words of common speech and to give them precise meaning, and in endeavouring to formulate exact generalizations and explanations of experience, formed hypotheses or theories, as is done in all the other natural sciences; and their statements about experience, and even their observations, were then apt to be distorted or perverted by their theories, which could at the best only approximate to the truth and which at the worst were very misleading. This tendency of psychology to diverge from the common and the literary traditions cul-

minated about the end of the 19th century, when the artificial character of psychology was carried so far by some of its exponents that their writings seemed to have no relation to human nature or experience and no bearing upon practical life. To some psychologists this was evidence that their science was still in a very crude and unsatisfactory condition; for they saw that the true purpose of psychology, in the progressive realization of which its justification must be found, is the improvement of our understanding, and therefore of our control, of human experience and behaviour. But others took a high line and reacted against the charge that psychology was a useless and purely academic pursuit, by retorting that it made no claim to be anything else; that it could have, and should not aspire to have, any bearing whatever on real life; that it was a game like any other, to be played according to accepted rules or conventions; that the enjoyment to be had from the game and the sharpening of wits that it might bring were its sole justification and *raison d'être*.

This stage, however, did not long endure. One of the most prominent of the 'strictly scientific' psychologists who dogmatically expounded this remarkable doctrine, in accordance with his principles and in defense of the strictly useless psychology which he had produced, shortly afterwards published a long series of books on the applications of psychology to life; and, as these books were not without value, they provided a convincing demonstration of the falsity of the doctrine. In thus turning from false theory to practical endeavour, this comically inconsistent psychologist exemplified the condition and the development of psychology at the time. For by the impartial enquirer the so-called science might well have been described as a mass of observations of the three kinds we have noticed, mixed with and distorted by a chaotic jumble of theories. Yet, in spite of this, it began to be apparent, even to some practical men, that this jumble of facts expressed by various writers in terms of the most diverse and inconsistent theories, though it might hardly deserve the name of a science, had yet

something to offer of value for practical life. And the psychologists kept plugging away, accumulating observations, refining their methods, and discussing their theories, vastly encouraged by these indications of a coming recognition and practical application of their hard-won knowledge. Nothing succeeds like success. And so these first successful applications of psychology have brought many more students into the field, have turned many into the paths of direct attack upon practical problems, *i.e.*, of applied psychology, and have excited a widespread popular interest in this now flourishing science. Even literary men are beginning to pay some attention to it and to find in it some help towards understanding and describing human experience and behaviour. Thus psychology is coming into its own; and the psychologist, instead of plowing his lonely furrow in the vague hope of contributing to a science that may some day be recognized as of value to mankind, finds himself embarrassed by the fact that men of the most varied occupations are calling on him for help, expecting from him definite pronouncements and safe guidance in a multitude of practical problems.

This revolution of the popular attitude towards psychology, from one of complete indifference to that of excessive trust and expectation of practical guidance from it, brings serious drawbacks and dangers to the orderly development of the science. But in the long run it must tend to its advancement. And we may now confidently say that psychology is destined to go forward, answering more fully the practical demands made on it, and gradually building itself up into the master science of human nature, a science which will provide for all the social sciences the foundation for lack of which they have long remained relatively sterile.

I have said that the theories of psychologists have coloured and often have distorted their observations and their descriptions. What, then, is to be our attitude towards theories or hypotheses in psychology? Are we to banish them and to build up a science without any hypotheses? That would be quite impossible, even if we were content to remain on the plane of purely descriptive psychology. For, in order

to describe, we must use general and abstract terms; and every such word implies a theory or hypothesis. Description requires classification of like with like; and the use of any word which denotes a class of like objects of thought implies the hypothesis that these objects are so far of common nature as to justify us in thinking of them as alike—for the particular purpose we have in view. Classificatory hypotheses of this kind are necessarily made by every science; and they have to justify themselves by their successful working. Without them, not only science but all intelligent discourse is impossible, whether about human nature or any other topic. The discussion of experience, of its conditions and its expressions, necessarily makes use of such classificatory hypotheses. But the question remains—Should we restrict the use of hypothesis as much as possible? Or is it advantageous deliberately to devise and use hypotheses that go beyond the bare minimum necessary for description, hypotheses of a more gratuitous nature, designed to explain the facts we describe? Here opinions differ very widely. Some authors profess a horror of hypotheses, and pretend to proscribe them from all science. This is mere ignorance and pedantry. We have seen that the hypotheses invoked in the use of general terms are inevitable; and the more far-reaching, more deliberately designed hypotheses, the use of which commonly involves the specialization or creation of some word, and which are commonly distinguished as explanatory, are not really of a different nature; for description and explanation are not really distinct processes; explanation is only description in more general and more abstract terms than simple description. It is all-important to be aware of what we are doing in making and using such hypotheses, and to be ready to modify or abandon them at need. If we maintain this attitude, hypotheses are great aids to discovery; and, in so far as they are well designed, they greatly simplify description and facilitate explanation.

When psychology began to separate itself as a science from the popular and literary traditions, it did not start with a clean sheet in respect of hypotheses. Not only was it

obliged to use and to revise, as best it could, the classificatory hypotheses of common speech; but also it found in these traditions certain hypotheses of the more far-reaching or explanatory kind, which, though vague and ill-designed for scientific purposes, were so firmly established that it was difficult to escape their influence. The very name of the science embodies such an hypothesis; for it implies that the science is concerned to study the soul or *psyche*, which for long ages had been commonly regarded as a vital principle inhabiting and animating each human body, and somehow the ground of each individual's experience. Many of these hypotheses were launched on their careers by great writers, or were developed by philosophers and, by their authority, established more securely in the common and literary traditions. Almost all psychologists make use of hypotheses having some such history. And the mischief is that there is no agreement among us as to which are the better of them; and, worse still, some hold them dogmatically, shape all their psychology upon them, and look with scorn upon all other types of psychology determined by the acceptance of other hypotheses. Wide divergences due to acceptance of widely different hypotheses are not peculiar to psychology. In physical science there are those who accept the ether, and those who do not; those who believe in absolute space and time, and those who do not; those who make use of the hypotheses of causation, energy, or force, and those who profess to have thrown them overboard as useless impedimenta. Similar differences in psychology are, then, no reproach to it and no ground for regarding it as in a peculiarly bad or hopeless position.

We may glance at some of the chief of these divergent types of psychology, resulting from different views as to the use of hypotheses.

Those who would restrict the use of hypothesis most severely aim at describing and explaining experience in terms of experience alone. They are allied with the pure idealists among the philosophers, for whom experience alone is real and all that exists or is real is experience. They are apt to

define psychology as the science of individual experience or of pure experience. Apart from the insuperable difficulties of the metaphysical doctrine implied, psychology of this type may confidently be said to condemn itself to relative sterility by its self-denying ordinance.

Ancient psychology accepted the soul and was chiefly concerned to distinguish the various functions of the soul and to assign them seats in the various parts of the body. In the modern period this type developed into what is generally called 'faculty psychology.' Experience was regarded as a function or activity of the soul, or of a part of the soul distinguished as the mind; or 'the mind' was used as synonymous with 'soul'; and experiences of each of the principal types, such as remembering, desiring, judging, comparing, were said to be due to the exercise of a corresponding faculty, a faculty of the mind or soul; each such faculty being a subsidiary hypothesis. Faculty psychology had a considerable career; and early in the 19th century it received a new lease of life in the modified form of phrenology, which taught that each faculty had its seat in some particular part of the brain. Both the older and the later form of faculty psychology have long been discredited. Even though it be admitted that the conception of a soul or mind, endowed with certain most fundamental faculties, is one that we cannot wholly dispense with; yet we must recognize that in itself this assumption can carry us but a little way, and that to multiply the faculties and merely ascribe each type of experience to the exercise of such a faculty is a fruitless procedure.

The great rival of the faculty theory was the *theory of ideas*. This theory has had a most varied and influential career in both philosophy and psychology; and, though no one has ever been able to make of it a consistent and intelligible theory, its influence still lives on. It is so firmly entrenched in the popular, the literary and the psychological traditions, that the word 'idea' is perhaps the most frequently used of all psychological terms, and few psychologists succeed in avoiding the use of it, even when they reject the theory it implies. In Plato's theory of ideas, each 'idea' was a super-

natural archetype of a class of things; and the 'ideas' existed in some remote and inaccessible region.¹ But at a later period 'ideas' were brought into the mind and changed their character. John Locke was chiefly responsible for establishing 'the new way of ideas' in modern psychology. He began by defining 'an idea' as anything whatsoever of which a man thinks.² And, since Locke accepted the common-sense view of the physical world, the view namely that material things exist independently of our minds, this definition seemed to identify 'ideas' and material things. But Locke's further discussion made it seem that 'the ideas' were somehow within, and a part of, the mind. The quick witted Irishman, Bishop Berkely, seized upon this inconsistency of the ponderous Englishman, and pointed out that, if all things of which we can think are 'ideas,' and if 'ideas' are in, or of, the mind, then all things of which we can think are in and of the mind; and, therefore, all things of which we can have any thought or knowledge are mental things. In this way he started a stream which swelled into that vast sea of troubled speculation called modern idealism. David Hume also took the 'ideas' into the mind; but, in doing so, made them a substitute for the mind itself. Locke's use of the theory of 'ideas' was a compromise between this extreme position and the faculty psychology; between 'the new way of ideas' and the old way of faculties. For he continued to use the hypothesis of the mind endowed with faculties, and described the mind as exercising its faculties upon or about its 'ideas.' But, said Hume, all we know directly is the stream of 'ideas' (and 'impressions', which for him did not differ essentially from 'ideas') and what we call the mind or soul is a mere empty stage which we unnecessarily assume to exist as the scene upon which the 'ideas' play their parts. From that time onward 'ideas' have played a very prominent part in psychology.³ Experience became a mere stream of 'ideas,' and

¹ This is the traditional interpretation of Plato's language, but in recent years it has been seriously questioned.

² "Whatsoever is the object of the understanding when a man thinks."

³ The more modern term 'presentation' is almost synonymous with 'ideas' and has the advantage of denoting also 'sense-perceptions.'

the course of experience was to be explained by the action of 'ideas' upon one another. Thinking of any object was described as 'having an idea of it'; and to think of it in a similar way a second time, or a third, was described as 'having the same idea again.' Then arose the question—Where was the 'idea' in the interval between my first and second thinking of the object? Two answers, often confused together, became current. One described 'ideas' as capable of existing in two conditions, the conscious and the unconscious conditions; when I am thinking of an object, my 'idea' of it is said to be in the conscious condition; when I cease to think of it, the 'idea' subsides into the unconscious condition. Some authors, seeing that the 'idea' was, if it was anything at all, essentially a section or piece of experience, sought to avoid the inconsistency of the expression 'unconscious idea' by supposing that, in the interval between its successive appearances, the 'idea' became, not unconscious, but only subconscious or very slightly conscious. The mind for this school was then no longer a stage or scene for the 'ideas'; but consisted in and of the mass of 'ideas,' unconscious or subconscious for the most part, but passing in turn into the conscious condition.

The other answer was that the mind, the stage on which 'ideas' play their parts, is also a store-house; that beneath the stage, which is illuminated, is a dark chamber in which 'ideas' are stored. From this dark chamber they issue in turn, to appear upon the illuminated stage; afterwards sinking down again into the dark store-house. Thus, for this theory, the mind consists of two chambers, one illuminated, one dark; the illumination is 'consciousness,' a sort of light which plays upon the 'ideas' as they emerge from below; the plane of emergence, that of the trap-doors in the stage, came to be spoken of as the threshold of consciousness, and the 'ideas' were said to rise or fall below this threshold, the transition from light to darkness being usually more or less gradual. This way of describing experience is full of inconsistencies and obscurities. And, though it may have played a useful part in the past, as for example in the psychology

of Herbart, there is no justification for continuing to use it. My own opinion is that any service performed by these confused and confusing fictions (namely, the 'ideas,' the dark and the illuminated chambers of the mind, the 'threshold of consciousness' and 'consciousness' as a light which illuminates 'ideas') is far outweighed by the vast mass of confused and loose thinking which they have engendered. They should be sternly banished to the psychological museums. Yet they still thrive; as we see in the Freudian psychology, which has flourished so greatly of late years, and in the usage of many writers of other schools.

Others define psychology as the science of consciousness, which is not far from the definition of it as the science of individual experience. For 'consciousness' is generally taken to be almost, if not quite, synonymous with 'experience'; although, as we have just seen, there are other usages of the word. 'Consciousness' is a thoroughly bad word; it has been a great misfortune for psychology that the word has come into general use. If it be used as synonymous with 'experience,' it must be admitted that 'experience' is much the better term; because, even when it is used in the substantival sense, it retains the form of the verb from which it is derived, so that we can hardly forget that experience implies someone who experiences and something which is experienced by the experienter. Whereas 'consciousness,' having the form of a substantive which cannot be used as a verb, allows us to forget that it stands for the fact of being conscious of something, and that it implies someone who is conscious of that something. And most of the writers who use the word allow themselves to fall into this error; though the etymology of the word should help them to avoid it. For it is derived from *con-scire*, which is the Latin for 'to know things together'; and 'consciousness,' if it is used at all, should be used to mean 'the act of knowing things together.' The word 'conscience' would have been a better word than 'consciousness' for psychological purposes, if it had not been appropriated by the moralists and given a special popular meaning. The French language is more fortunate than ours in that it retains the word 'conscience' in its original sense.

The havoc wrought in psychology by the word 'consciousness' is chiefly due to our deep-seated tendency to reify (to make a thing or a stuff of) every object that we think and speak of, especially when we think or speak of it by aid of a word of substantival form. Yielding to this tendency, very many writers have fallen into the way of thinking and speaking of (consciousness), *i.e.*, of the act of knowing things together, as though it were a stuff. Then, on contemplating this stuff, it appears obvious that it is not a simple stuff of uniform texture, but rather highly complex and variegated; more like a mosaic, or a piece of tapestry, than a uniform white sheet. Moreover, it appears like a stuff that perpetually changes, as though an endless strip of tapestry were drawn along before our eyes. Therefore, proceeding to analyse this stuff, they seem to themselves to discover that it is made up of small pieces of stuff; and they then set themselves to find the smallest particles, the irreducible elements or atoms, of this stuff. The question then arises whether all these particles, of which 'consciousness' is supposed to be made up, are of one kind; and, while some maintain that they are of several or of many kinds, the majority assert that they are fundamentally of one kind only, which they call sensations or sensory elements. And some speculators of a metaphysical turn go further and say that these particles are not really the ultimate elements; but that, just as the atoms of the older chemistry are now commonly said to consist of electrons, or of particles of electricity or of some other stuff, so the sensory elements or atoms really consist of still finer particles of 'consciousness' which they like to call 'mind-stuff' or 'mind-dust.' And some of them go so far as to assert that, not only ourselves or our minds, but the whole universe is made up of such mind-dust, arranged and compounded in many different conjunctions. And then, in order to make plausible this strange view, the view that the seas and mountains, stars and planets and all things else are made up of this stuff, they fall back on the expressions 'subconsciousness' and 'threshold of consciousness' and 'consciousness as a light which illuminates one chamber of the mind and all that enters into it'; striving

in this way to make intelligible their fantastic doctrine that the stuff they call 'consciousness' exists in two forms, namely, 'conscious consciousness' and 'unconscious consciousness.'

This way of treating of experience readily combines with 'the way of ideas.' For 'ideas,' regarded as things, are a product of the same tendency to reify whatever we think of. The 'consciousness psychologist' accepting 'ideas' as things, as innocently as he accepts consciousness as a stuff, asks himself—Of what stuff are these things called 'ideas' composed? The answer is not far to seek. Obviously, 'ideas' are composed of 'sensory elements'; and so arises a full-blown 'sensationalism' or sensationist psychology. It is generally held by the exponents of this doctrine that it has the great merit of solving, or abolishing, the perplexing problem of the relations between mind and matter; and, since matter is said by them to be composed of the same stuff as 'consciousness' or 'ideas,' they proudly claim for it the title of 'idealism.'¹ Anyone who is capable of contemplating the history of psychology in a detached spirit will be inclined to agree that this 'consciousness' is not only stuff but also non-sense. And some of the exponents of 'sensationalism,' having carried the doctrine as far as they could, have themselves arrived at this conclusion. For, turning round upon their own doctrine in a moment of illumination, they have asked—Does this stuff that we call 'consciousness' really exist? Does consciousness exist? And they have been constrained to reply—No, obviously it does not exist; it is an illusion, a fiction.

Having thus displayed the purely fictitious and fallacious character of 'consciousness psychology,' some of the more thorough-going thinkers have deduced from its failure a new philosophy which they call 'neo-realism.' Thus has been evolved the very latest form of materialism which, instead of saying, with the old-fashioned materialists, that the brain

¹ The word 'idealism' has two entirely different meanings which are often confused implicitly and sometimes deliberately. In the one meaning it denotes any doctrine which asserts that the whole world consists of mind or minds or mental operations. In the other meaning it stands for any moral attitude of striving to realize an 'ideal'—to make real or actual a state of affairs that seems to us better than the existing state.

secretes 'consciousness,' says rather that the distinction between 'consciousness' or experience and things known or experienced is fictitious and illusory, that 'ideas' and 'perceptions' and all the other things said to be composed of bits of 'consciousness,' are really identical with the objects of the physical world. Thus the wheel comes full circle, and we are brought to the position from which Locke started out — 'ideas' are once more the things of which a man thinks; that is to say, they are identified with all 'the furniture of earth,' while 'the choir of heaven,' including all such things as minds, souls, selves, or spirits, with all their modes of activity and experience, is left out from the picture of the universe.

Most of the exponents of 'consciousness' and 'sensationalism' do not achieve this last turn of the wheel. And in order to understand their position, which is perhaps the one most widely held at the present day, we must consider a very important influence in modern psychology of which up to this point I have made no mention, the influence namely of physiology.

The ancient philosophers knew nothing of the functions of the brain. But, early in the modern period, it was made clear that the functions of the human brain are very important, and that they are somehow bound up with our knowing and feeling and striving, with our experience in short. Therefore, many ardent spirits set to work to solve the secrets of experience by investigating the brain. An immense and ever-increasing amount of human energy has been devoted to this very difficult research, and a multitude of facts have been established. And the number of those who seek to explain all human experience and activity in terms of the structure and functions of the brain, or of the nervous system, has constantly gained in numbers and influence. The program of this school was crudely formulated in the 18th century by La Mettrie's notorious dictum that the brain secretes thought as the liver secretes bile. But various more-detailed formulations were made by the physiologists, according to the type of psychological doctrine

which they chose to adopt for assimilation with the facts of their own science.

Early in the nineteenth century, the phrenologists, of whom Francis Gall was the leader, combined a crude brain-physiology with the 'faculty psychology.' Even before this date the 'idea psychology' had been given (by David Hartley 1749) a physiological turn which greatly added to its plausibility, and which went far to make the fortune of that type of 'idea psychology' long dominant in England and known as 'the psychology of association.' The 'association psychology,' of which Hobbes, Locke and Hume were the founders, and of which Hartley, the two Mills and Bain were the leading exponents, claimed to explain all experience as a succession of 'ideas,' which were said to be linked or associated together and to draw one another along into consciousness by means of their links or automatic couplings. Now, though 'ideas' seemed to be facts or real things, it was difficult to give any adequate picture of the links or couplings; and therefore Hartley, one of the first of the associationists, devised, no doubt with a sense of great relief, the plan of attributing to the brain the couplings and all the pullings and pushings, in short the whole business of shunting and shifting 'ideas' into and out of 'consciousness.' And the more the brain was studied, the better it seemed to lend itself to this purpose.

The doctrine of localisation of cerebral functions, which in the crude form of Gall's phrenology had proved untenable, seemed to become more definitely established as knowledge grew rapidly in the later part of the nineteenth century. The brain became revealed as a vast jungle of cells connected by a tangle of nerve fibres, each of which cells seemed to be in some sense a vital unit. The 'idea psychology' was then translated into physiological terms by supposing that each 'idea' had its home in some one cell, where it dwelt at peace and in darkness during the greater part of its time; and that occasionally, when a nervous current reached the cell, the 'idea' within it glowed and became 'conscious' or was projected into 'consciousness.'¹ And it was conceived that,

¹ T. H. Ziehen (*Outlines of Physiological Psychology*) is the best contemporary exponent of this view.

whenever the excitement of one cell followed immediately upon that of another, the two cells became linked together by a path of low resistance; so that thereafter the excitement of one would easily spread over to the other, leaving the path between them still more open. In this way all experience was physiologically explained as the successive emergence of 'ideas' out of their holes or cells according to the law of neural habit. Then, when 'sensationalism' analysed 'ideas' and displayed them as made up of 'sensory elements' or 'sensations,' these elements or atoms were assigned each to its own nerve-cell; and the 'idea' or 'cluster of sensations' had then for its neural counter-part a cluster of cells which worked together as a functional unit. And this unit could grow by the inclusion of more cells, as the 'idea' could become more complex by the inclusion of more 'sensory elements'; and the group of cells could become linked with others to form a group of groups, which might come into action simultaneously or successively.

To those who accepted this scheme it naturally seemed that there was no longer any need to postulate a 'mind,' a 'soul,' a 'subject of experience,' a 'self'; an experiencer or a thinker of any kind who thinks, desires, remembers, expects or in any way experiences, was deemed unnecessary; or, if it seemed convenient at times to use language of this kind, it was sufficient to say that 'the idea' of the moment does the thinking, or that the passing thought thinks and is the only thinker.¹ But, in the main, the active mode passed out of fashion in the description of experience; and the laws of thought became the mechanics of the brain. 'Consciousness,' it was said, was reduced to the rôle of a passive spectator. But even this was an overstatement of the position assigned to it; even a spectator is active to the extent of observing. It would be more true to say that 'consciousness' was given the rôle of a 'super.'

The full persuasiveness of this translation of psychology into terms of brain mechanism is only to be understood when

¹ This last was the subterfuge preferred by James, who in his great work 'The Principles of Psychology' made an heroic effort to work out a consistent 'Sensationism.'

it is realized that, in addition to providing what seemed an equivalent in the brain for each 'sensation' or other discoverable element of consciousness, and a plausible scheme of the way in which these brain-elements might be supposed to play upon one another, physiology was working out at the same time a scheme which claims to be able to explain, in principle, all human action in terms of the mechanics of the nervous system. This scheme is a development of the principle of *reflex action*, which was first clearly formulated by the great philosopher Descartes at the opening of the modern period. Descartes suggested that the principle would suffice to account for all the bodily activities of animals; but he himself and most other psychologists continued to regard the bodily actions of men as expressions of their experience, as somehow determined by their thinking, by their feeling, their desiring, their remembering, their recognizing, and so forth.

Descartes' account of reflex action was merely a brilliant guess; but the subsequent course of physiological discovery has justified it abundantly. It has been shown that, in both men and animals, some elementary but seemingly purposive movements may be evoked, even when the brain is wholly out of action or destroyed and the individual remains unaware of the whole process. Thus, if the sole of the foot is pricked, most men and most animals will quickly draw away the foot. Common sense says—'He feels the prick and draws away the foot in order to avoid the painful impression.' Well, physiologists have shown that this withdrawal of the foot may occur in a very similar way, when the spinal cord has been separated from the brain. And a man in this condition, though he may see his foot move when pricked, does not feel the prick or the movement; and he knows nothing of it, if his eyes are closed. They have shown that the prick excites a nerve in the foot; that this excitement spreads up the nerve to the spinal cord as a wave of physical change (not unlike a current of electrical change in a telephone-wire) leaps across from the sensory nerve to a motor nerve (much as the electric spark leaps from one terminal to another) and so issues along the motor nerve and, reaching the muscles of the leg, causes

in them an explosion which makes them contract and so withdraw the foot. That is the type of reflex action as conceived by Descartes and studied by modern physiologists. The latter have shown that many simple movements and secretions may be produced in animals and in the human organism in this relatively simple way; the whole action or reaction being a sequence of processes which seem, in principle, capable of being adequately described and explained in terms of physics and chemistry; although as a matter of fact we are still far from having achieved such a description.

Physiologists have shown also that the spinal cord (the part of the central nervous system which lies in the spine and to which all the sensory nerves pass in and from which all the motor and gland-exciting nerves issue) consists essentially of a large number of such mechanisms for effecting reflex actions; each mechanism being a more or less complicated conjunction of sensory with motor nerves, by means of which the nervous current excited in any sensory nerve is led over into some group of motor or gland nerves. Further they have found reason to believe that the whole nervous system, including the brain, is built up on the pattern of these reflex mechanisms of sensori-motor nervous arcs, which lead on from sense-organs to the executive organs, namely muscles and glands. The brain seems, in fact, to consist merely of a multitude of such nervous paths or arcs, differing only from those of the spinal cord in being longer and more complicated, and having more abundant connexions with one another. The mysterious 'nerve-cells,' which had been supposed to be the abodes of 'ideas,' are shown to be merely swellings upon the fine protoplasmic threads which are the nerve fibres. Those of the surface or cortex of the brain (the famous grey matter) seem to be in no way essentially different from those which occur in the spinal cord on the simplest reflex arcs; and their essential function seems to be the regulation of the nutrition and growth of the nerve fibres which are parts of the same cells. Thus all human action is made to appear to be of the type of reflex action, to be the issue merely of the play of nervous currents, started in the sense-organs by stimulations from the physical

world and propagating themselves through the jungle of the nervous system, finding always the paths of least resistance according to purely physical principles. All human action is reflex action, or, as the principle is more commonly formulated, every human action is a mechanical response to a stimulus.

If you, my reader, are new to thinking of this sort, you may say—That is all very well, but how about memory? Surely many of my actions are prompted and guided not by sense-impressions, but by memory, by memories of past experiences! The physiologist has his answer to this question also. The nervous system contains many reflex paths innately organized; that is to say, they develop and become ready to lead off sensory excitation to the appropriate muscles spontaneously; just as the blood vessels develop spontaneously in such a way as to lead the currents of blood wherever they are needed. But, unlike the network of the blood vessels, the nervous network is plastic. Every reflex arc is connected with a multitude of others; so that, under the most favourable conditions (as when for example all nervous pathways are rendered very open by a large dose of strychnine) excitement started in any sensory nerve may spread like an avalanche throughout a multitude of channels and throw practically every muscle in the body into violent action. Any nervous current is normally restricted to comparatively few pathways, leading it over to a few muscles, by the fact that every sensory nerve-path is more intimately connected with certain motor nerves than with any others; but these connexions or junctions between nerves or neurones (as the complete units, consisting of nerve-cell and fibre together, are now called) are very delicate structures; they are now generally called ‘synapses.’ Each synapse presents a certain resistance to the passage of the current of nervous energy. But this resistance is not a fixed quantity; it varies from moment to moment, according to the conjunctions of many influences that play upon it. Hence the unpredictable and variable character of all reactions, save the simplest reflexes. Of all influences that affect the resistance of the synapse, the

most important is conceived to be the actual passage of the nervous current across it from neurone to neurone; for, it is held, this renders the resistance of that synapse permanently lower than it previously was, leaves it with a diminished power of resisting the passage of the current. Granting this, imagine that an entirely novel odorous object is presented to your nose; this 'stimulus' excites by way of well-organized reflex paths the reaction of biting. The object then stimulates your mouth or tongue in such a way as to excite another reflex action, that of spitting it out (in common parlance, we say—The object has a pleasing odour, but a bad taste). Now suppose further that a similar object is presented to your nose on a second occasion. It is probable that you will not bite, but rather will make (incipiently at least) the motion of spewing it out. Common sense would say that you remember the bad taste and that this leads you to refrain from repeating your bite; or it might say that, owing to your prior experience of the bad taste, the odour is now unpleasant to you; or that it now, instead of exciting your appetite, excites disgust. But the physiologist says that the succession of the two reactions on the first occasion has established a more intimate connexion between the two reflex paths through which the two reactions were evoked, by lowering the resistance of certain synapses; so that now the nervous current excited by the odour, instead of flowing on to the muscles which do the biting, flows over directly to the second set of muscles which effect the movement of spewing out. Thus an alteration of resistances at certain synapses has produced a new reflex path. A reaction acquired in this way is called a 'conditioned reflex.' All that used to be called 'profiting by experience,' or 'intelligent adaptation of behaviour,' or 'the acquirement of habits,' or 'association by contiguity in time,' in short, all facts that used to be classed under the terms 'memory' and 'habit,' we are now to regard as essentially a matter of the establishment of 'conditioned reflexes.' No wonder that in these days the young student of psychology swears by 'conditioned reflexes' and is apt to regard the term as the key to most of the riddles

of the universe, or at least as the master key of human fate! Having grasped this master principle, he feels, and in some quarters he is encouraged to feel, that he need no longer rack his brains over the traditional puzzles of psychology. For it has become clear to him that love, honor and duty, faith, hope and charity, reason, will and moral effort, are merely so many names by which we denote as many varieties of 'conditioned reflex,' of somewhat complicated pattern no doubt, but not essentially different from the scratch-reflex of the dog's hind-leg. He sees clearly that the good dog is the one whose 'conditioned reflexes' lead him to the softest spot and the best bone. And equally clearly he sees that the good man is he whose conditioned reflexes have been established by a judicious system of rewards and punishments; and that the wise man is he whose conditioned reflexes lead him to avoid pain and to pursue pleasure. *Sic itur ad astra.*

Having arrived at this point of his career in the pursuit of understanding, two courses lie open to the aspiring student of human nature; he may attach himself to either of two schools. If he is of a severely practical turn of mind, he will give ear to the 'behaviorists.' The official representatives of this flourishing school will tell him that he need trouble himself no further over the problems of experience or of 'consciousness'; need not ask the troublesome questions—Does consciousness exist? Can consciousness do this or that? How is it related to the reflex processes of the brain? He will be told that the conduct or behaviour of any man is merely the sum of his conditioned reflexes; that this is all we know and all we need to know. But, accepting this position, he has one more choice still to make. He may elect to fall in with the crowd marching behind Mr. J. B. Watson¹ and to say with him—What has been called experience or consciousness may occur or exist for all I know or care. But I am not interested in it. I am concerned only to understand human behaviour. I know that all behaviour is mechanically determined by reflex processes; let me get on with the study of 'conditioned reflexes.'

¹ 'Psychology from the standpoint of the Behaviorist.'

Or, if he feels the need of a metaphysical justification of his position, he may follow the forlorn banner of Mr. E. B. Holt,¹ the most thorough-going of the neo-realists, who with a sublime consistency will tell him that what he has been accustomed to call his experiences or his 'consciousness' of pleasure or of pain, of desire, of effort, of knowing, of remembering and the rest, that all these are really movements of particles or of currents of energy in the world about him; and who will explain to him how and why, from the dawn of reflexion to the end of the 19th century, the whole human race has been the victim of the illusion that thinking of a material object is not identical with the existence of that object, but rather a fact of a different order.

But the student who has accepted the principle of the conditioned reflex as all-sufficient may nevertheless find it impossible to appreciate Mr. Holt's extreme position; and he may also refuse to copy Mr. Watson in his devil-may-care attitude towards experience. For he may feel that his experience is too real to be explained away as an illusion; and he may argue that, even though behaviour be merely a sequence of reflex actions mechanically determined, yet we cannot afford to repudiate introspective observation entirely. For, he may reflect, our knowledge of these reflex processes is still rudimentary and schematic only; and, if 'sensations' or 'sensory elements' faithfully attend the workings of the brain, each drawn along by some 'conditioned reflex' like a slave at the chariot-wheel of his master, we may hope, by continuing the study of them, to throw more light upon these theoretically so clear, but practically so obscure, master-factors of human nature, the 'conditioned reflexes.' If he takes this line, he will fall in with a large and excellent company of psychologists who are following just this road. Not all of these pin their faith to the conditioned reflex conceived in the strictest and narrowest sense; but for all of them (and probably this group comprises a majority of the academic psychologists of the present day) human conduct is determined wholly and strictly by the mechanical action of the bodily organs, and chiefly by that

¹ 'The Concept of Consciousness.'

of the nervous system; that is to say, by physical and chemical changes in the nervous and other tissues, processes which are in principle strictly predictable and describable as movements in space, obeying laws or conforming to equations no other than those which are found to hold good for inorganic processes. For them the nervous system has taken the place of the soul, or mind, or self, or the subject of experience; the brain, or at most 'the passing thought that thinks,' is the sole thinker, and thinking is mechanical brain-action or its 'epiphenomenon.' A large number of psychologists who agree in this respect (though they differ widely in other matters) do not, like the late Hugo Muensterberg, loftily assert that, because 'sensations' or other elements which compose their 'consciousness' are artificial abstractions, their psychology has no bearing upon human life and no practical value; nor do they, like Mr. Watson, cease to take an intelligent interest in their own introspective findings and those of other observers. They rightly recognize that, even if their physiological assumptions be true, it is nevertheless a fact that physiological research into the functions of the brain has been greatly aided in the past by introspective observations; and, though they anticipate that the mechanistic physiology of the brain will more and more supersede, and ultimately wholly absorb, psychology, they hold that its advance may be greatly aided by the continued and more refined study of experience. Professor Titchener may perhaps be named as standing unambiguously for this type and program of psychology.¹

Now let us go back to consider the student confronted by the consequences drawn from the study of the nervous system. He is told that his experiences are merely mosaics of sensory elements, each of which is called into being, or into 'consciousness,' by a physico-chemical process of the brain; that his belief in his power of choosing, resolving, striving, of seeking the good and ensuing it, in *purposive action* in a word, is a

¹ 'A Beginner's Psychology.' Mr. B. Russell in his 'Analysis of Mind' appears for the moment as the latest exponent of psychology of this type. He has reduced it to the lowest level of simplicity and banality.

delusion; that his hardest choice is not, as the older determinists used to say, determined by his strongest desire, but is not at all influenced by desire or by anything of that nature; and that even pleasure and pain, which for so long were widely held to be the springs of all human action, have no influence upon it.

We have seen how, confronted with these conclusions, which are supported by an imposing array of authority, and feeling themselves unable to criticise the observations and reasonings upon which they are based (for indeed the full appreciation of these would demand several years of special physiological study, which few psychologists find time to undertake) the majority capitulate, and follow one or other of the lines I have briefly sketched.

But some (and I hope that this book may add to their number) will hesitate, remembering vividly perhaps some devastating conflict of desires, some moral struggle hardly won, some intense pain, some base temptation, some impulse of profound pity or of tender devotion, of fierce anger or horrible fear. They may ask themselves—Is there not something radically wrong with a system of thought which tells me that these experiences are of no account in the world? Must there not be some flaw, some ill-founded premise or assumption, in the argument which leads to this incredible conclusion; a conclusion which runs counter to the teachings of the moral leaders of all ages; which represents Man as no more capable of creative activity or of self-determination than the humblest animal or the crystal in the test tube; which regards the Bible, or the works of Shakespeare, of Beethoven, of Newton, as fortuituous concourses of atoms, whose arrangement in space is due to mechanical processes of natural selection no other than that which sorts the pebbles from the sand upon the beach?

It is said that he who hesitates is lost. But sometimes it happens that he who hesitates is saved, especially when it is a question of dashing down a steep place. Let the beginner in psychology who hesitates at the brink of the slope that leads to materialism and a rigid determinism look round

again and critically survey the ground. Let him also look down the slope and there in imagination he may see the sturdy figure of T. H. Huxley, struggling in vain in his old age to lay the spectre he had so confidently helped to create;¹ there he may descry the forlorn figure of Herbert Spencer, once acclaimed the king of mechanists, but now remembered as the author of a 'chromo-philosophy' of scandalous vagueness; there also he may discern Ernst Haeckel, the dogmatic apostle of a mechanical evolution, going down to the grave with a lie upon his lips. Let him turn and look upward, and he will see the serene figures of Charles Darwin and Newton and Faraday, of Hermann Lotze, of Leibnitz and Plato and Wordsworth, and of all the great poets, an august company of great men who refused to 'lay the intellect to rest upon a pillow of obscure ideas,' whose voices still ring down the ages, insisting that Man is more than matter and may yet be master of his fate.

Thus warned and encouraged, the student may be prepared to give due weight to the following reflexions. All the varieties of psychology which propose to abdicate in favour of a mechanistic physiology, which propose to replace the hypothesis of a mind, a soul, a self, a subject, an experiencer, by that of a brain or a bodily organism working on strictly mechanical or physical principles (that is to say, behaviourism, sensationism, associationism, presentationism), may be conveniently classed together as mechanistic psychologies.² All of these are based upon certain assumptions which, though they have a certain plausibility, have not been justified by the best efforts of those who make use of them so confidently. The chief of these assumptions are: (1) that mechanistic physiology will at some remote date prove adequate to the task that lies before it, namely, the working out of a complete

¹ 'Evolution and Ethics.'

² These psychologies are not necessarily mechanistic. Any one of them might be combined with a belief in the reality of purposive action. But they all lend themselves readily to alliance with the mechanistic view of organisms which denies the reality of purposive action; and as a matter of fact are usually so allied. Behaviorism especially seems to be deduced implicitly from the mechanistic dogma and to lie, somewhat uneasily, in this metaphysical bed.

description and explanation of the bodily processes of organisms (including the human organism) in terms of the mechanistic principles of physics and chemistry; (2) that it is, or may become, possible to give an intelligible account of the relation between the facts of experience and the facts of behaviour, in terms compatible with such mechanistic physiology.

Let us examine briefly these two assumptions. The assumption which underlies mechanistic physiology is part of a wider twofold assumption which underlies the whole of mechanistic biology; namely, that the mechanistic description of the inorganic world is in principle correct, and that organisms have been evolved from inorganic matter by the operation of natural selection, without the intervention of any influences, forces, or modes of action, other than those operative in the inorganic world. Although this assumption has been very often and very confidently made, its validity appears more doubtful now than it did a generation ago. It is beginning to be generally admitted by leading biologists that Darwin's great principle of natural selection will not suffice to account for organic evolution and for that appearance of purposive adaptation of organisms to their environment which forces itself on our attention throughout the realm of life. As our knowledge of living things grows, the problem of life and its evolution appears more, rather than less, resistant to mechanical principles. The same is true within the narrower field of general physiology. The researches of the last one hundred years have brought us a vast mass of knowledge about the physical and chemical processes that go on within the body; but each new step reveals more problems of regulation and adaptation than it solves. We are told by some of the more philosophically minded leaders in physiological research¹ that we seem to be moving ever further from the goal of the mechanistic program, rather than approaching it. Again, within the still more special field of the physiology of the nervous system, our knowledge is astonishingly defective in view of the confidence with which

¹ E. g., Dr. J. S. Haldane, 'Life, Mechanism and Personality.'

it is put forward as a substitute for psychology. We do not know the nature of the change which propagates itself along the nerve fibres, as the so-called nervous impulse; and opinion is acutely divided as to whether this is of one, or of two, or of many kinds. We know nothing about the nature of the inhibitory process which is involved in all coordination of nervous activity. We know next to nothing of the peculiar constitution and processes of the all-important synapses. The doctrine of 'specific energies of sensory nerves', which, under several successive forms, has been as a ray of light in a great darkness, is now much blown upon and most by those who have no intelligible substitute for it. The doctrine of the localisation of functions in the cerebral cortex, which, about the beginning of this century, seemed to be well-established, at least for the elementary sensory and motor functions, is also rocking insecurely and seems to need at the least a complete recasting in some form not yet suggested.¹ The facts of restitution of function, after destruction of tissue, remain as completely refractory to mechanistic interpretation in this as in all other fields of biology.²

As regards their second great assumption, the mechanistic psychologies are in no better case. The proposal of the behaviorists 'to look this problem boldly in the face and pass on' cannot be acceptable to any man not entirely lacking in scientific curiosity; for, even though experience be nothing but a kind of phosphorescence upon the brain, it is yet that without which life would have no value and no meaning for us. The proposals of the neo-realists would, if carried to their extreme, abolish the problem; but only one author has had the moral courage to attempt to carry it to this ridiculous extremity. The 'sensationalists' are divided between, or vacillate uncertainly between, the doctrine of *epi-phenomenalism*, which would make 'sensations' a kind of phosphorescence called into being by the nervous currents traversing the cortex, and the mind-stuff theory, according

¹ Cf. S. I. Franz, Presidential address to the American Psychological Association, *Psych. Rev.*, March, 1921.

² *Ibid.* See also 'The Science and Philosophy of the Organism' by Hans Driesch, for discussion of facts of this order.

to which 'sensations' are things which exist in their own right as the primordial stuff of which the world is composed. Both doctrines utterly fail to give any intelligible account of the conditions under which these 'sensations' come together to form 'consciousness' as we know it introspectively, the conscious activities of perceiving, imagining, remembering, or otherwise thinking of an object.¹ And both fail to carry through the program whose simplicity is the main strength of the mechanistic position, namely that of describing and explaining the whole of nature in one set of terms or categories, the categories of physical science. For, if the matter of the brain can generate 'sensations', that matter is more than, and other than, the matter or physical processes described by physical science; and, if all matter is 'sensations,' or of the same nature as 'sensations,' which can combine to constitute the various forms of experience, then the account of it which physical science gives is most misleading, and the categories of that science, however useful they may have been, are only distant approximations to the truth.

Indeed, in the present state of physical science, it is absurd to pretend that its categories must prescribe the type of all legitimate description and explanation. For these are in perpetual flux. In the palmy days of materialism, it was common form to assert dogmatically that the universe consists of atoms of matter, that all energy was the momentum of such atoms, and all change the transmission of momentum by impact of one hard and resilient particle upon another. But those days of the 'billiard-ball-universe' are gone forever, save perhaps in the imagination of a few belated biologists who have picked up their views of physical science from old-fashioned text books. All the categories of physical science, matter, energy, motion, momentum, mass, and Space and Time themselves, are in question; and no man can say

¹ Prof. C. A. Strong ('The Origin of Consciousness') has made the most thorough and sustained effort to carry out the 'mind-stuff' program; but finds himself constrained to introduce into his sketch of the universe 'given essences' which are, 'logical entities or subsistents . . . not either physical or psychological' and which seem to be so inconsistent with the general program as to constitute an admission of the impossibility of working it out consistently.

whether any one of them will emerge alive from the fermenting chaos of modern physical speculation.¹

This being the state of affairs, it is surely premature, to say the least of it, to assume that human nature and human action are capable of being adequately explained or described in terms of the categories of physical science. The only wise course for the psychologist, as for the biologist in general, is the bold one of asserting the relative independence of his science and his right to choose and use the categories that are most profitable for his purpose, the better understanding and control of human nature. Let him leave to the future the most difficult task of harmonising the conclusions of the biological and of the physical sciences, while keeping an open mind and a critical attitude towards all attempts in that direction.

In this book, then, I shall attempt to show my readers how we may describe human nature and human experience, and, in a proximate (*i.e.*, in a scientific rather than a metaphysical) sense, explain the facts, by the aid of working hypotheses which make no claim to be true ultimately or metaphysically, but claim merely to be useful at the present time. The hypotheses to be adopted must determine the method of description; and the language of description will imply these hypotheses.

I shall try to avoid language implying the hypotheses which I have adversely criticised in the foregoing pages,

¹ Let the student ponder the following words with which a leading physicist (Prof. A. S. Eddington, professor of astronomy at Cambridge) concludes a recent work on 'Space, Time and Gravitation.' "The theory of relativity has passed in review the whole subject-matter of physics. It has unified the great laws, which by the precision of their formulation and the exactness of their application have won the proud place in human knowledge which physical science holds to-day. And yet, in regard to the nature of things, this knowledge is only an empty shell—a form of symbols. It is knowledge of structural form, and not knowledge of content. All through the physical world runs that unknown content, which must surely be the stuff of our consciousness. Here is a hint of aspects deep within the world of physics, and yet unattainable by the methods of physics. And, moreover, we have found that where science has progressed the farthest, the mind has but regained from nature that which the mind has put into nature. We have found a strange footprint on the shores of the unknown. We have devised profound theories, one after another, to account for its origin. At last, we have succeeded in reconstructing the creature that made the footprint. And Lo! it is our own."

namely—'ideas,' 'consciousness' as a stuff compounded of 'sensations' or other elements or units of composition, and mechanical reflexes as the units of action. It is not easy to adhere strictly to this program, for the language of 'ideas' and 'sensations' is common, not only to most psychology, but also to the literary and the commonsense traditions; and I must pray for indulgence if here and there I should lapse into these convenient and familiar but misleading modes of speech.

I will indicate at once in briefest outline the hypotheses that seem to me most acceptable and the terms which we may most profitably use. First—What is to be our attitude toward physiological knowledge, especially our knowledge of the structure and functions of the nervous system? We have seen that many psychologists at the present day have given up altogether the use of the terms 'mind,' 'soul,' or 'subject.' Recognizing that the mind can be profitably described neither as merely a bundle of faculties, nor as a more or less organized mass of 'ideas' regarded as enduring things that pass in and out of 'consciousness,' they have substituted for 'the mind' the nervous system or the brain; with the results that we have glanced at. Now, if we refuse to follow this modern practise, we must go back to, or adhere to, the older practise and must still speak of 'the mind.' For what the older psychology meant by 'the mind' was that *something* which expresses its nature, powers, and functions in two ways: (1) the modes of individual experience; (2) the modes of bodily activity, the sum of which constitutes the behavior of the individual. The mechanistic psychology says—this *something* which expresses its nature in these two ways is essentially the brain, as described to us by the mechanistic physiology. There can be no question that we are bound to postulate this *something*; and that, if we are not content to regard it as merely the brain mechanically conceived, we must have some name for it, and must recognize that it has a very complex nature, or is a very complex organisation; that it first manifests itself in the relatively simple behaviour of the new-born infant; and that it grows in complexity and

definiteness, partly in virtue of its innate or hereditary tendencies, partly in virtue of the influences that play upon it from within and without the body, and of the modes of activity with which it responds to these influences.

I do not think that we can find a better word to denote this *something* than the old-fashioned word 'mind.' 'The Mind,' then, of the individual organism is that which expresses itself in his experience and in his behaviour; and we have to build up our description of the human mind by gathering all possible facts of human experience and behaviour, and by inferring from these the nature and structure of the Mind. I say nature *and* structure; for we may usefully distinguish between these. It seems probable that Mind has the same nature wherever and whenever it exists or manifests itself, whether in animals, men, or superhuman beings, whether in the new-born infant, the fool or the wise man. On the other hand, the structure of the mind seems to be peculiar to each individual; not only is it very various in the various species of animals (if they have minds) and in man; but the structure of the mind of one man is different from that of every other man; and, in any one man at each stage of his career or life-history, it is not quite the same as at any other stage, because, as we said, it constantly develops or evolves into greater complexity and definiteness during the years of growth, and later perhaps, in extreme old age, undergoes a regressive change or involution.

The mechanistic psychologist will say—What you call 'the Mind' is just what I call 'the brain.' Why go out of your way to set up this vague mysterious purely hypothetical *something* which, as you admit, no one has ever seen or handled or can hope to see or handle, while all the time you have the brain, as solid and as real as a lump of cheese, which we positively know to be concerned in all experience and in all behaviour, and about which the labour of thousands of expert workers has built up an immense mass of knowledge? To this I would reply—I do not underrate the value of this physiological knowledge and research; but I assert that, in the present state of science, it is not profitable to substitute the brain

for the mind. To do so limits unduly our freedom of thought; it ties us down to one kind of explanation, leads us to absurd consequences (of the kind we have noticed), and, worst of all, is apt to blind us to facts of observation, and biases our interpretation of other facts.

On the other hand, I have no sympathy with certain psychological purists who would banish all physiological facts and theories from psychology, on the ground that psychology and physiology are two distinct sciences whose facts and categories can not be mingled without confusion. I would say—Let the psychologist make the fullest possible use of all the help that physiology can give; let him regard it as one of his tasks to harmonize or coördinate the facts and descriptions of his own science with those of physiology. But let him not capitulate to the unjustified demand that his science shall abdicate its functions in favour of a mechanistic physiology. Ultimately we may learn how to harmonize the findings of the two sciences far more satisfactorily than it is possible to do at the present day. Let us push explanation in terms of nervous structure and function as far as ever we can; but in doing so, let us not suppose that such explanations are ultimately and completely true.¹

After thus defining what meaning we are to attach to the word 'mind,' we may return to the question of the proper or most useful definition of psychology. Provisionally we defined it as the study of human nature, admitting that this is unsatisfactory in that it seems to claim the whole of human physiology as within the province of psychology; whereas it is clear that at present it is to the best interests of both

¹ I have myself followed this plan and indulged in a good deal of physiological speculation. To the young student of psychology who intends to make psychology, either as pure science or in any of its applications, his business in life, I would say—learn all you can of physiology and especially of the nervous system. It is worth while to devote several years to this study of physiology while you are still young. It is noteworthy that some of the greatest psychologists have followed this plan, and have made themselves acquainted not only with physiology but with the whole range of medical studies of their day. I may cite the names of John Locke, R. H. Lotze, Wm. James. It may be claimed also that some experience of the actual practice of medicine is of great value to the psychologist. In no other way can he come into the most intimate contact with other persons and study them from all points of view.

sciences that they shall be kept distinct, the nature of their relations being left for better and more exact determination as both sciences progress. Psychology clearly is concerned with human nature in its mental aspect; physiology with human nature in its bodily aspect. At present there are many facts of importance to the one science, which the other can afford to neglect. If it is objected that this division of labour implies the old view of radical dualism of mind and matter, or soul and body, reply that it need not and should not carry this implication; that this question of dualism is a metaphysical problem with which science is not immediately concerned, one which it may and should leave undecided; that, since no certain answer can be given to it, science should not hamper itself by accepting dogmatically, or even provisionally, one or other answer.

With this understanding, then, psychology may be defined as the science of the human mind; we may make the definition more exact by adding the words 'positive' and 'empirical'; 'empirical' to mark the fact that psychology relies upon the great method common to all the natural sciences, namely, observation of concrete facts and the induction of general rules or laws from these concrete particulars, rather than upon deduction from any *a priori* principles; 'positive' to mark it as the science of mind as it actually exists and operates, to distinguish it from the sciences primarily concerned with the ideals, norms, standards, or rules of right thinking and conduct.¹

¹ As long ago as 1905 I suggested that psychology might be defined as the positive science of conduct or behaviour (in my 'Primer of Physiological Psychology'), and in later works I have used and defended this definition. I preferred this definition to any of the others current for two reasons: (1) it seems to mark off the field of psychology clearly from the fields of other sciences and to include everything that properly pertains to psychology; whereas ambiguities, and logical and other difficulties, are involved in all the other definitions; (2) it lays stress on the importance of the objectively observable facts, the facts of outward behaviour or bodily action in which mental activity expresses itself; and these facts had usually been unduly neglected in favour of the other class of expressions of mind, the introspectively observable facts. I still regard this definition as a good one, logically perhaps the best. But since my suggestion was made, it has been adopted and carried to an extreme by the 'behaviourists.' In protesting against a too exclusive study of the introspective data, and rightly insisting on the importance of the study of behaviour as a psychological method, they have overshot the mark and swung into the untenable position noticed above.

The psychologist has, then, to build up his description of the human mind by inference from the observed facts of behaviour, the behaviour of men and of animals, and from the observed facts of experience, facts of his own experience observed introspectively and facts of others' experience described and recorded by them.

This is the fundamental kind of psychology, the general psychology presupposed by all the more special kinds of psychological study, such as the study of individual or racial peculiarities of mental constitution, the study of group life or collective psychology, the study of abnormal or disordered minds. Sometimes the name of one or other of the principal methods of study is prefixed to the word psychology, as though this denoted a specialised form, such as experimental, or physiological, or comparative or genetic, or introspective; but, though these are convenient divisions, justified by practical exigencies of study, they are not logically distinct branches of the science; and the student should not let himself be misled into thinking of them in that way. Indeed the special branches themselves must also be regarded as capable of lending valuable aid in the attack on the problems of general psychology; and in so far they may be regarded as methods of study, rather than as distinct special branches of the science.

In the main our minds have been evolved in the course of our efforts to comprehend and control our physical environment. We deal with this most successfully, for all practical purposes, by regarding it as made up of enduring things which Hence I am disposed to say "If you are going to get on, it's time for me to get off." This question of the definition of psychology is after all one of minor importance. All our lines of separation between the sciences are provisional only and liable to readjustment as knowledge grows. We ought to aim not so much at perfectly logical definitions, which will only be possible when the sciences are completed, but at definitions stated in terms of the practical purposes with which we pursue the several sciences. Accepting this principle, we may define physiology, as it exists today, as the science which aims to give us better understanding and control of the chemical and physical processes of the various organs of the body; and psychology as the science which aims to give us better understanding and control of the behaviour of the organism as a whole. This is the demarkation proposed in my 'Psychology, the Study of Behaviour,' Home University Library, 1912.

undergo changes and act upon one another and upon us. In consequence, when we turn to the effort to describe experience, we tend to treat it in similar fashion, as though it were made up of things: whatever phase we think of we make an object of our thinking; and we tend to think of every object of our thinking as a thing. Whereas experience is not made up of things; it is a process and perhaps in all cases a train of activity.¹

The most general and fundamental facts about experience as we know it, or enjoy it, are two. First, experience or experiencing is always an experiencing of something, it is always a thinking about some object, even when, as in psychologizing, that object is itself an experiencing or thinking. Secondly, all experiencing or thinking is the experiencing or thinking of *some one*, some subject, some person, some organism. So far as we positively know, this *some one*, this subject, is always a material organism; or is embodied in, and manifests itself to us only in and through the medium of, a material organism. That is to say, experience, as we know it, is always the thinking of some subject of or about some object. Therefore I propose to use the verb 'to think' as the most general word for the description of experience. This usage is wider than the usual one; but it has good precedent in Descartes' famous phrase '*Cogito, ergo sum*'—*cogito* or 'I think' is the most general form of experience. If we use this word, we shall be more likely to avoid the error of reifying our mental processes; for it will constantly remind us that, whenever we refer to a fact of experience, we imply *some one* thinking of *some thing*.

¹ Prof. Bergson has made much of this tendency to reify whatever we think of; he describes it as a natural and unavoidable weakness of the human intellect which unfits it to deal with the facts of life and experience. He recognises a mode of mental activity which he calls 'intuition' and regards as fundamentally distinct from intellect or intelligence; and he assures us that this faculty alone is capable of coping with the facts with which psychology is concerned. I have never been able to comprehend the nature of this alleged faculty, nor am I convinced by his arguments against the competence of intellect. But I do recognize the need of conducting our intellectual processes with the greatest circumspection in this sphere, and of choosing our language in such a way as to give as little scope as possible to this falsifying tendency to reification.

Let the student ask himself whether he has ever chanced to find 'a percept,' or 'a concept,' or 'an idea,' or 'a sensation' lying about loose in the world; as he may find a pebble, or a star, or a bone, or any other physical thing. He will realize that he has never done so, nor is likely to do so. He might as well expect to find 'a falling' or 'a movement' without something that falls or moves, as 'a perceiving,' or 'a remembering,' detached and isolated from the subject who perceives or remembers. And surely, to say 'I think of X,' or 'I perceive X' is a more direct and accurate statement than to say 'I have an idea of X' or 'I have a percept of X,' or than to say—'An idea or percept of X has come into my mind or risen into consciousness.' The former is a plain and simple statement of fact; the other modes of expression are circumlocutions that imply strange and highly disputable theories, such as those we have glanced at in this chapter. Even if we use these perverted traditional expressions, we have to admit, when the question is raised, that this 'idea' or 'percept' or 'concept' and the 'consciousness,' into and out of which it is said to pass, always belong to some one, to some person or other organism,¹ which we naturally denote either by a proper name or by a pronoun, I, you, he, she; or it. All experience then is the experience of some subject. Whether all experience takes the form of thinking of an object is a debatable question. It has been pointed out that, while every experience that any one can introspectively observe and report upon is a thinking of an object, yet sometimes we seem to be almost purely passive, to approximate to mere suffering or enjoying without being aware of any object; and it has been argued that we may justifiably postulate a kind of experience that, going yet further in this direction, becomes a pure passivity. Such hypothetical purely passive experience would not be thinking of an object and might be called 'anoetic experience.'

¹ Some modern philosophers and logicians speak of our acts of thinking as 'ideas' and 'concepts,' and 'concrete universals' and 'neutral entities' and so forth; and thus, misleading themselves into the belief that such entities exist, they are led to the monstrous conclusion that whatever we can imagine has or enjoys actual existence—all of which is a natural outcome of the 'idea' theory, which in turn is merely the outcome of a loose usage of words.

By adhering to the direct and natural form of speech, we may secure another great advantage, namely, we may distinguish clearly between *facts of mental activity* and *facts of mental structure*. Mental structure is that enduring growing framework of the mind which we infer from the observed manifestations of mind in experience and in behaviour; and, since this develops, grows and, even when the mind is at rest, endures, we may properly describe it and its parts in substantival terms, which terms we shall have to select and define with care. The distinction between mental structure and mental process or functioning or activity is comparable to that between the structure and the functioning of a machine or of a bodily organ such as a muscle. It is of course impossible to find any true analogy or parallel to illustrate the task of the psychologist in building up his account of the structure of the mind; but the following crude illustration may help to make it clearer. Suppose a mechanical toy or doll, actuated by a complicated clockwork, which reacts in a variety of ways under different conditions; and suppose that the investigator, who wishes to ascertain the nature of the machinery within it, can only watch its movements under various conditions and hear a variety of sounds emitted by its mechanisms when they are in motion. From the data of these two kinds he could build up by inference some account of the structure of the hidden mechanisms. It is one of the allurements of the 'idea theory' that 'ideas' are treated at will as either functions or parts of the structure, a very convenient procedure; but one which leads to endless confusion. Many other words are frequently used in this ambiguous and confusing manner, which we must try to avoid.

In speaking of mental structure and likening it, as I did just now, to the structure of a machine, we must not be misled into taking the word 'structure' in the sense of a material structure or any spatial arrangement of parts. We commonly and properly speak of the structure of a poem or of a musical composition, meaning a whole consisting of parts in orderly functional relations with one another; and, though the structure of the mind is not of the same order as these

structures, yet these, rather than the material structure of a machine, should be thought of as offering the closer analogy. Those students who find it useful to picture, or visualise, or represent in diagrammatic form, whatever they think about may visualise the structure of the mind in spatial terms without danger, if they bear this warning in mind. And, in accordance with what was said above of the use in psychology of knowledge of the nervous system, we may, without serious danger, allow schematic diagrams of nervous structure to represent features of the mental structure, if we hold fast to the truth that the mind and its processes cannot be literally translated into, or adequately represented by, or supplanted by, any description of the structure and functions of the nervous system mechanically conceived. The structure of the mind is a conceptual system that we have to build up by inference from the data of the two orders, facts of behaviour and facts of introspection.

A NEW FORMULA FOR BEHAVIORISM¹

BY EDWARD CHACE TOLMAN

University of California

The idea of behaviorism is abroad. In the most diverse quarters its lingo, if not its substance, is spreading like wildfire. Why?

In the first place, it is to be observed that ever since the days of Ebbinghaus's experiments on memory the inadequacy of the merely introspective method as such has been becoming more and more obvious. And the recent work in mental tests and animal psychology has strengthened this conviction. In the second place, there has always been a formal logical difficulty about the introspective method which has troubled certain minds. That is, the definition of psychology as the examination and analysis of private conscious contents has been something of a logical sticker. For how *can* one build up a science upon elements which by very definition are said to be private and non-communicable? And, thirdly, the introspective method is practically arduous and seemingly barren of results. It is these three features, then, which seem to have been primarily responsible for the spread and catching of behavioristic categories.

What, now, does the behaviorist offer as a substitute? We turn to the arch-behaviorist, Watson. Behaviorism, he says, will be the study of stimulus and response such that given the stimulus we can predict the response, and given the response we can predict the stimulus. Very good! But how does he define stimulus and response? He defines them, he says, in the terms in which physiology defines them; that is, stimuli are such things as "rays of light of different wave lengths, sound waves differing in amplitude, length, phase

¹ Read in part before the Western Psychological Association, Berkeley, Calif.; August 5, 1921.

and combination, gaseous particles given off in such small diameters that they affect the membrane of the nose," etc., and responses are such things as "muscle contractions and gland secretions."¹ We turn, however, to a later chapter² and read with astonishment, in a footnote, that "It is perfectly possible for a student of behavior entirely ignorant of the sympathetic nervous system and of the glands and smooth muscles or even of the central nervous system as a whole, to write a thoroughly comprehensive and accurate study of the emotions." But how can this be, we ask, if, by very definition, behavior is a matter of 'muscle contractions' and 'gland secretions?' How, on the basis of this definition, can a person 'ignorant of glands and muscles' write a behavioristic account of anything? That he can write such an account we would admit. The only difference between our point of view and Watson's would be that we should insist that such an account would be the only truly *behavior* account, and that an account in terms of muscle contraction and gland secretion, as such, would not be behaviorism at all but a mere physiology.

It should be noted that the possibility of a behaviorism which shall be not a mere physiology but something different has apparently already occurred to a number of writers. Thus, for example, Holt says that "the phenomena evinced by the integrated organism are no longer merely the excitation of nerve or the twitching of muscle, nor yet the play merely of reflexes touched off by stimuli. These are all present and essential to the phenomena in question, but they are merely the components now—the biological sciences have long recognized this *new and further thing and called it 'behavior'.*"³ Mrs. de Laguna also explicitly states that what we want is a behaviorism which is not mere physiology. "In order to understand behavior we must resolve it into a system of interrelated *functions*, just as in order to understand the physiological workings of the human body we must envisage

¹ 'Psychology from the Standpoint of a Behaviorist,' p. 10.

² *Op. cit.*, Chap. VI, 'Hereditary modes of Response: Emotions,' p. 195.

³ E. B. Holt, *J. of Phil., Psychol., & Sci. Methods*, 1915, 12, 366.

the complex of chemical and mechanical processes as falling into such fundamental groups as digestion, circulation, etc., constitutive of the physiological economy. Now just as there is a physiological economy, so there is a larger vital economy in closest union with, yet distinguishable from it. This is the system of behavior, by means of which the being, animal or human, maintains his relations with the environment and forms a factor in its transformation. The science of behavior has the task of tracing the lineaments of this larger economy."¹

A. P. Weiss also seems, to some slight extent at any rate, to lean towards this same view of the desirability of a non-physiological behaviorism. For example, the following: "The investigation of the internal neural conditions form part of the behavioristic programme, of course, but the inability to trace the ramification of any given nervous excitation through the nervous system is no more a restriction on the study of effective stimuli and reactions in the educational, industrial or social phases of life than is the physicist's inability to determine just what is going on in the electrolyte of a battery while a current is passing, a limitation that makes research in electricity impossible."²

The two essential theses which we wish to maintain in this paper are, first, that such a true non-physiological behaviorism is really possible; and, second, that when it is worked out³ this new behaviorism will be found capable of

¹ Grace A. de Laguna, *PSYCHOL. REV.*, 1919, 26, 410-411. See also other articles by the same author. 'Dualism in Animal Psychology,' *J. of Phil., Psychol. & Sci. Methods*, 1918, 15, 617-627; 'Dualism and Animal Psychology, A Rejoinder,' *J. of Phil., Psychol. & Sci. Methods*, 1919, 16, 296-300, and 'Empirical Correlations of Mental and Bodily Phenomena,' *J. of Phil., Psychol. & Sci. Methods*, 1918, 20, 533-541.

² 'The Relation between Physiological Psychology and Behavior Psychology,' *J. of Phil., Psychol. & Scientific Methods*, 1919, 16, 626.

³ Attention should be drawn to two other very significant attempts to begin a detailed 'working out' of such a behaviorism in addition to Mrs. de Laguna's in the article on 'Emotion and Perception from the Behaviorist Standpoint' already quoted from. These are to be found in a series of articles by J. R. Kantor: 'A Functional Interpretation of Human Instincts,' *PSYCHOL. REV.*, 1920, 27, 50-72; 'Suggestions toward a Scientific Interpretation of Perception,' *PSYCHOL. REV.*, 1920, 27, 197-216; 'An Attempt towards a Naturalistic Description of Emotions,' *PSYCHOL. REV.*, 1921, 28, 19-42, and 120-140; 'A Tentative Analysis of the Primary Data of Psychology,'

covering not merely the results of mental tests, objective measurements of memory, and animal psychology as such, but also all that was valid in the results of the older introspective psychology. And this new formula for behaviorism which we would propose is intended as a formula for *all* of psychology—a formula to bring formal peace, not merely to the animal worker, but also to the addict of imagery and feeling tone.

But how can this be done? By what single common set of concepts can we possibly take care both of the facts of gross behavior and of those of consciousness and imagery?

Before attempting to suggest such a set of concepts, let us indulge in a preliminary epistemological skirmish. Let us start from the usual dualistic hypothesis implicit in traditional psychological thinking. Suppose, that is, we assume that consciousness is (for the purposes of psychology at any rate if not for those of an ultimate metaphysics) a new kind of something or other which is added to certain behavior situations but not to others. Introspective psychology claims the study and analysis of this new something or other as its own peculiar field. Consciousness is assumed by it to be something private to each individual which he alone can analyze and report upon. And the introspective account purports to be such an analysis and report. What now can our behaviorism answer to this? Our behaviorism will reply that whether or not there is such a private something or other present in the conscious behavior situation and lacking in the unconscious one, this private something or other never 'gets across,' as such, from one individual to another. All the things that do 'get across' are merely *behavior* phenomena or the objective possibilities of such phenomena. Suppose, for example, that I introspect concerning my consciousness

J. of Phil., 1921, 18, 253-269. And in a series of articles by R. B. Perry, 'A Behavioristic View of Purpose,' *J. of Phil.*, 1921, 15, 85-105; 'The Independent Universality of Purpose and Belief,' *J. of Phil.*, 1921, 18, 169-180; 'The Cognitive Interest and its Refinements,' *J. of Phil.*, 1921, 18, 365-375. It must be pointed out, however, that whereas both these authors are giving yeoman strokes in the direction of just such a non-physiological behaviorism as the writer is contending for, neither of them seems himself to be wholly self-conscious of this essential difference between such a true behaviorism and a mere physiology.

of colors. All you can ever really learn from such introspection is whether or not I shall behave towards those colors in the same ways that you do. You never can learn what the colors really 'feel' like to me. It is indeed conceivable that just as immediate 'feels' (if there are any such things) the colors may be something quite different for me from what they are for you, and yet if I agree with you in behaving to them; *i.e.*, in my namings of and pointings to the colors, no amount of introspection will ever discover to you this fact of their uniqueness to each of us as immediate 'feels.' You will only discover what the colors are for me as behavior possibilities.

Let us now turn to some of the actual concepts which seem to me to be required by such a point of view. We will confine ourselves to four: *stimulating agency*, *behavior-cue*, *behavior-object*, and *behavior-act*. They may be thought of as very loosely analogous to the physiologist's concepts of external stimulus, receptor-process, conductor-process, and effector-process.

The *stimulating agency* may be defined in any standardized terms, those of physics, of physiology, or of common sense, and it constitutes the independent, initiating cause of the whole behavior phenomenon. Thus on different occasions it may consist variously in, and be describable as, a sense-organ stimulation (in the case of perceptual behavior), as the administering of a particular drug, *e.g.*, hasheesh (in the case of hallucinatory behavior), or as the neurological end-result of a preceding activity (in the case of a behavior based upon memory or recall).

The nature of the *behavior-cue* will be understood most readily from a consideration of the dialectic which underlies the experimental work on sensory discrimination in animals. In such work the results, when strictly interpreted, are found to tell us nothing but the possibility of differences of behavior as a result of different stimulating agencies. If, for example, we find that a mouse can learn to behave differently as result of blue and yellow stimuli but not as a result of a red and green stimuli, we do not conclude anything as regards

the animal's consciousness of these colors, as such, but merely something as regards the behavior-cues which these colors are capable of evoking in him. That is, blue and yellow wave-lengths are capable of producing in him two different behavior-cues, whereas red and green wave-lengths are capable of producing in him only one. In other words, where the older psychology talked about sense-qualities our new behaviorism will talk about behavior-cues.

The new concept is identifiable with the older one in so far, but only in so far, as the latter explained the possibility or lack of possibility of differences of *behavior*. The new concept departs utterly from the old in so far as the latter implied something concerning 'immediate feels' as such. By applying different stimulating agencies to our organism we discover the number and range of his possible behavior-cues. We learn which stimulating agencies he can use as a basis for differences of behavior and which he can not use as cues for different behaviors. And we learn something concerning the degrees of difference between these different behavior-cues. For example, we learn that, in a human of normal color vision, although the stimulating-agencies designated as orange and red wave-lengths produce behavior-cues which are different from one another, still these behavior-cues are more similar to one another (in that, on occasion, they are more likely to lead to an identical behavior) than are the two behavior-cues produced by the stimulating agencies known as red and green wave-lengths—and so on. In other words the sum of the behavior-cues possible for any given organism constitutes a total system which is to be defined not merely in terms of its relation to the stimulating agencies which evoke its members, but also in terms of the interrelations of similarity and difference between those members. We do not learn, however, anything about sensation-qualities, as such, neither when we observe the gross behavior of another organism nor when we ask the latter to *introspect*. We learn the nature of his behavior-cues. We do not learn the nature of his 'immediate feels.'

Let us turn now to a consideration of the next of our four concepts, that of the *behavior-object*. Just as the concept of the behavior-cue was found to bear a certain relation to a concept of the older psychology (viz., that of sense quality) so the concept of the behavior-object bears an analogous relation to another concept of the older psychology; viz., that of the perceived or apperceived *meaning*. A behavior-object results from a behavior-cue or a group of behavior-cues which, because of a particular behavior situation, possesses for the organization in question a specific behavior-meaning. For example, we present an ordinary western European with a chair, it produces in him, because of the structure of his sense-organs and as a result of its color, shape, etc., certain specific behavior-cues. In addition, however, because of his particular training and past experience and state of behavior-readiness at the moment such behavior-cues resulting from these shapes, colors, etc., arouse in him a very specific group of behavior-tendencies; e.g., those of sitting upon, getting up from, kneeling on, moving up to the table, etc. This group of aroused tendencies defines his behavior-object. That is, they constitute on that particular occasion the behavior-meaning of the colors, shapes, etc.

To use the terminology of the older psychology we would say that the behavior-cues in question are here apperceived as the behavior-object chair. On another occasion, however, this same group of behavior-cues might be apperceived not as a chair, but as a very different sort of behavior-object. If we were drunk, it might be apperceived, not as a thing to sit on, to kneel on, but as a thing to run away from, to scream at, etc. Thus, the behavior-object is to be defined in the last analysis simply in terms of the group of behaviors to which it may lead. And it is to be emphasized that it, no more than the behavior-cue, can be defined in terms of 'immediate conscious feels.' For no one of us ever knows for certain what another organism's 'conscious feels' may be. We know only the behavior implications of those conscious feels.

We turn now to the last of our four concepts—that of the *behavior-act*. The behavior-act is simply the name to be given to the final bits of behavior as such. The behavior-act together with the stimulating agencies constitute the fundamentals upon which the rest of the system is based. They are such entities as to 'sniff,' to 'sit,' to 'scratch,' to 'walk,' to 'gallop,' to 'talk.' They are directly correlated with the action system of the given organism. They vary and increase in number with the growth and development of the organism. But it is they alone which, at any given stage in this growth and development, tell us all that we know of such an organism's 'mentality' (even when that organism is another human being who can 'introspect'¹). Used as a means of comparing different stimulating agencies on the basis simply of the relative discriminability and non-discriminability of the latter, the behavior-acts provide us with our definition of behavior-cues (*i.e.*, sensation and image qualities). And used to discover the totality of different alternative behavior which may result from a given collection of behavior-cues, the behavior acts provide us with our definition of behavior-objects (*i.e.*, perceptions and ideations).

If, now, we sum up the situation it will appear that the problems for our behavioristic science must fall into three groups: those of (1), given the stimulating agency, determining the behavior-cues, (2), given the behavior-cues, determining the behavior-object, and (3), given the behavior-object, determining the behavior-act. The first of these problems is the well known one of the older physiological psychology of determining the relations between sensory and image qualities and their underlying physiological conditions. The second problem, that of the relation of behavior-object to behavior-cue, is the old one of perception and apperception. Our re-wording of it will not, I think, make it any the less easy of final solution. Finally, the problem of the relation of behavior-act to behavior-object is the extremely important problem of *motive*. It is the problem of desire, emotion, instinct, habit, determining set. It is a problem which the

¹ Such introspection is itself but one of these behavior-acts.

older analytical formulation tended to obscure and make almost impossible. If our behavioristic formulation has any practical value at all—if, that is, it has any value in addition to that of unifying under a single rubric all the different types of method which psychology employs, then that practical value will be, I believe, in the more successful treatment which it will allow and suggest for this matter of motive, determining set, and the like.

What, finally, are we to say about those difficult, and to the opponents of behaviorism, seemingly insuperable problems of imagery, feeling-tone, language, introspection? An adequate discussion would cover many pages. I can here merely throw out a suggestion or two. In the first place I would suggest that consciousness as such, *i.e.*, conscious behavior as opposed to merely unconscious behavior, is to be thought of simply as the case in which a *number* of behavior-acts are being made or tending to be made simultaneously. If I am *conscious* of the chairness of a chair, it is because I tend not only to sit, but to stand up, to kneel, etc., simultaneously. If, in addition, I am conscious of the color and shape of the chair as such, I tend, am set, not merely to behave in these appropriate ways toward chairs but also to discriminate by all other possible behaviors its particular color and shape from all other colors and shapes.

Images and ideas would be simply a particular case where behavior-object and behavior-cue have different space and time implications from those holding in the case of presented objects and qualities. And feelings and emotions would be treated as combining both behavior-objects and behavior-cues in that they involve both discriminable qualities and specific unvarying types of behavior (for example, approach, avoidance, and the like). Finally, language in general and introspection in particular are simply themselves behavior-acts which in the last analysis indicate to the observer the very same behavior-cues and behavior-objects which might be indicated by the mere gross forms of behavior for which they are substitutes.

In closing this very brief and inadequate sketch it may

be remarked that its excuse is to be found in the hope that it may have suggestive and propaganda value, if nothing else. The five points I should wish to emphasize are:

1. There are obvious formal inconsistencies in the subjectivistic formula as such.

2. The possibilities of a new non-physiological behaviorism have already found expression on the part of a number of writers.

3. Such a non-physiological behaviorism seems to be capable of covering not only behaviorism proper but introspectionism as well. For, if there are any such things as private mental 'feels' they are never revealed to us (even in introspection). All that is revealed are potentialities for behavior.

4. As a first step in working out such a non-physiological behaviorism I suggest the concepts of stimulating agency, behavior-cue, behavior-object, and behavior-act. And,

5. The value of the new formation will be in part theoretical, in that it will bring under a single rubric all the apparently different and contradictory methods of actual psychology; but in part, also, practical, in that it will allow for a more ready and adequate treatment of the problems of motive, purpose, determining tendency, and the like, than was made easy by the older subjectivistic formulation.

INTELLIGENCE AND BEHAVIOR

BY A. A. ROBACK

Harvard University

The conception of intelligence took a behavioristic turn with the functional notion introduced by Binet. The definition of intelligence as adaptability to new situations at once became popular among psychologists and is still, I believe, the accepted view. Up to the time of Binet, intelligence used to be understood in the sense of comprehension, that is in the etymological sense. Intellect, again, was a term employed to designate a higher kind, perhaps a more abstract type, of intelligence.

From the inundation of intelligence tests and the importance attached to the testing of school children one should have judged that it is the easiest thing in the world to describe the nature of intelligence. The Thorndike-Spearman disagreement as to the modality of intelligence has exposed certain perplexing features surrounding the general question: "What is intelligence?" At variance with one another are also the definitions of intelligence as given by leading investigators in that field. For Thorndike, intellect is 'the power of good responses from the point of view of truth or fact.'¹ Terman, on the other hand, says, "An individual is intelligent in proportion as he is able to carry on abstract thinking."² In the first definition, we undoubtedly have a behavioristic slant, although Thorndike does not strain himself to eliminate all reference to mental activity from his discussion. The second definition, which follows pretty closely that of Meumann, is decidedly mentalistic, as is also Freeman's view.³

¹ E. L. Thorndike, 'Intelligence and its Measurement, Symposium,' *J. of Educ. Psychol.*, 1921, 12, p. 124.

² L. Terman, *loc. cit.*, p. 128.

³ F. N. Freeman, *loc. cit.*, pp. 133-134.

Colvin's earmarks of intelligence as that which an individual possesses 'in so far as he has learned, or can learn to adjust himself to his environment,'¹ is, as he suggests, very much like the received definition among psychologists and probably savors a bit of behaviorism, though the word 'learn' contains a good deal which may be taken either way. Pintner's definition differs from Colvin's only by the greater emphasis laid on 'relatively new situations in life.' Yet Pintner seems to be no thoroughgoing behaviorist, considering that he speaks of 'mental tests calling into play the synthetical and analytical activities of the mind, reasoning, judgment and the like.'² Henmon's view is that 'intelligence is intellect *plus* knowledge' which is amplified by the statement that "intelligence is indicated by the capacity to appropriate truth and fact as well as by the capacity to discover them."³ From this as also from the tenor of his whole discussion it may be inferred that he is not particularly favorable to the behaviorist position. Of all the thirteen members who participated in the symposium, so far as I can see, only two show a decided leaning towards a behavioristic interpretation of intelligence, and a third may be favoring it slightly. Of these, Peterson is of the opinion that "it is a mechanism for adjustment and control, and is operated by internal as well as by external stimuli,"⁴ and Dearborn believes that the commonly accepted definition of intelligence can now be adequately described 'in terms of the current "objective" psychology,'⁵ while Haggerty takes the position in his contribution that "Intelligence is descriptive of behavior and not of static component parts of the 'mind.' When one conceives of the mind as a 'state' or a 'structure,' the word intelligence becomes meaningless. The implication of *activity* is essential in the concept of intelligence."⁶ Yet I am not sure that Haggerty would refuse to conceive intelligence in

¹ S. S. Colvin, *loc. cit.*, p. 136.

² R. Pintner, *loc. cit.*, p. 140.

³ V. A. C. Henmon, *loc. cit.*, p. 196.

⁴ J. Peterson, *loc. cit.*, p. 198.

⁵ W. F. Dearborn, *loc. cit.*, p. 210.

⁶ M. E. Haggerty, *loc. cit.*, p. 213.

terms of mind. In his definition at the outset intelligence is made out to be 'a practical concept of connoting a group of complex mental processes. . . . If I am right in taking it that Haggerty takes exception only to the static representation of mind and advocates a dynamic view instead, we may observe that only two out of the thirteen participants have been convinced as to the satisfactory application of behaviorism to the notion of intelligence.

The only direct allusion to behaviorism in the symposium is to be found in Woodrow's paper, in which he regards intelligence as a capacity to be envisaged in neither purely mental nor purely behavioristic terms. "Intelligence cannot be defined in purely mental terms, because the capacity for acquiring valuable modes of mental functioning is itself not mental. . . . On the other hand, intelligence cannot be satisfactorily described in purely behavioristic terms, because (I mention only one reason, one which is persistently overlooked by behaviorists) while the degree to which behavior is intelligent is simply the degree to which it attains success, success has no real meaning except by reference to some want, desire, intention, plan or purpose."¹ But if intelligence cannot be described in purely behavioristic terms, what shall we do with the residue except to reinstate it under the mental régime?

The definition of intelligence as a capacity or power, it seems to me, suffers from insufficient analysis of the concept. It is in this defect that Woodrow's dilemma originates. A capacity of mind is not mind. Very well then. Is it the physical basis of mind, the nervous constitution? Then why not define intelligence as the nervous constitution or integration suitable for bringing about certain responses? The truth of the matter is that when we say our friend is an intelligent man, we don't mean to designate the basis of this intelligence, but simply to convey the notion that whenever *X* has occasion to perceive, discriminate, judge, infer, etc., he does so well, *i.e.*, he is borne out, in most cases, by subsequent events. It would scarcely do to wrest a word out of its setting in the

¹ H. Woodrow, *loc. cit.*, pp. 208, 205.

minds of the millions of people who have used it and to give it an entirely different meaning which is foreign to the intention of the many intelligent people who have made the term current. When I am speaking about a coin, I should not welcome the suggestion that I mean metal inasmuch as a coin is made of some kind of metal. Intelligence originally meant understanding, comprehension; and it is in the sense that we employ the term now, too. Presumably intelligence testers are referring to the same concept as laymen when they speak of intelligence, or else the layman's concept which is perfectly understood in conversation deserves separate treatment and its seniority demands priority of attention over the technical term in vogue among intelligence testers.

Too much stress has been laid on such auxiliary words as capacity, power, ability, etc., in disregard of the fact that such words derive their actual meaning from the substantive with which they are coupled. Horse-power, candle-power and power of response are instances of three different applications of the concept power. The first two have their justification in the physical sciences; the third is merely an analogy.

The word response is even a more unfortunate interloper. On account of this empty concept, the significant core of the issue is lost to sight. The imbecile who notices and counts the different shapes of rocks by the sea-side '*from the point of view of fact or truth*'¹ is responding to the situation just as well as, if not better than, the scholastic philosopher of the mediæval age who discussed both *pro* and *con* the question whether a prostitute could by divine dispensation become a virgin again. Yet even from our modern enlightened viewpoint, because of his greater comprehension and vastly more complicated reasoning processes, which we could follow, the scholastic would be regarded as intelligent and the imbecile not. It is the thinking that is important; a response which is not the direct outcome of higher mental activity is negligible as a criterion of intelligence. Why then mix the wheat with the chaff for the sole purpose of reducing both to one

¹ See Thorndike definition, *Supra*.

category, as if without the word response, no one should have surmised that our thought might have been provoked by some situation? More than that, often the earmarks of intelligence are to be found in the sole fact that there is at all a response, whether good or bad, to a stimulus which an unintelligent person would never recognize as such. The true thinker, and perhaps it will not be taken amiss if I regard a thinker as intelligent, will often *create the stimuli himself*. *The creation of the stimulus is, in such cases, the response* and a much more intelligent response it is than many a successful move prompted by external influence. *To see a problem sometimes denotes a higher degree of intelligence than to solve one.*

The misplaced emphasis laid on such auxiliary words as 'ability' and 'response' is comparable with the grammatical stress placed on the word 'loan' in the school-boy's sentence structure when he asks 'for the loan of a knife.' What he wants is the knife and not the loan. So, too, speaking of the intelligent man we mean that *his higher mental operations are as a rule carried out on some more or less consistent plan*. The degree of intelligence can be determined only as a result of comparison of an individual's consistency and versatility in thinking with the mental activity of other people in respect to these two traits.

In definitions of intelligence the word 'capacity' occurs, I should think, figuratively as an enduring condition or perhaps, better, reference. If an intelligent person had occasion to display his intelligence every minute of his conscious life, the reference to his *capacity* to think or to learn or to respond would be a redundancy. The story told about the plaintiff who brought suit against his neighbor for wanting to call him a liar is quite pertinent here as illustrative of the situation. "But how do you know he wanted to do that?" he was asked. "Well," was the reply, "if he didn't *want* to call me a liar, he wouldn't have said, 'You are a liar.'"

Much temptation as there is to view it as a capacity for responses or adjustments, the mentalistic conception of intelligence is still on the ascendant. The qualification of biological or behavioristic terms by the word mental is by no

means insignificant. What is a mental response but a mental process or set of processes, once we recognize that the response part is frequently a behavioristic fiction, since in life, problems are not always given but many a time created by the 'respondent'? The same with slight modifications may be said about mental adjustments. Determining what it is that one must adjust himself to is the sign of intelligence more pronouncedly than the adjustment itself. We must finally then insist on the point already made that intelligence is characterized by mental processes rather than by motor or glandular activity.

As we approach the typically behavioristic interpretation of intelligence we are confronted by a number of utterances that betray the specific presuppositions already exposed in the course of this discussion. Treating of the nature of mentality, Wieman rather startles us by pronouncing mind or mentality to be 'a certain mode of doing things on the part of an organism.'¹

The incongruity between the terms so complacently linked together will be evident upon a close scrutiny of the sentence. Is it not somewhat novel, this idea of modality of organismal activity? Again, to set forth that the organism performs the mental operations, as is implied by Wieman, is on a par with holding that the body thinks! Mind, one would suppose, is extra-organismal, for an organism is, by definition, the living being with its physical structures and functions. Mental activity cannot properly be considered within that scope, unless we are to so extend the denotation of the term as to cause confusion between the original biological concept and its psychological superimposition. Yet Wieman is within his rights, seeing that he accepts the seemingly more fallacious alternative and enlightens us further to the effect that 'mental effort is nothing else than organic effort' in a very delicate and tentative form. We have occasion here to note the fallacy of identifying coördinates.² What havoc the word adjustment plays in modern radical theories of mind would

¹ H. V. Wieman, 'The Nature of Mentality,' *PSYCHOL. REV.*, 1919, 26, 230.

² H. N. Wieman, *loc. cit.*, p. 240.

prove an interesting piece of research. An instance of this multiple use appears in the same article where the observation is made that 'if every tendency of the organism were perfectly adjusted to every other, mentality would disappear.' Passing over the indefiniteness of the phrase 'perfectly adjusted,' which would require considerable explanation before the nature of the process could be understood, we should arrive at the conclusion that Wieman is stating a truism, or else he is guilty of a flagrant *petitio principii*. If his protasis means that all desires, wishes, purposes, impulses, etc., were to cease—if that is what he understands by a perfect adjustment, then he might as well have said 'if all mental activity were to cease, all mentality would disappear'; for the only criterion of a perfect adjustment is necessarily the absence of tendencies like desire or purpose. More instructive would it have been to transpose protasis and apodosis in such a way as to bring out the converse, viz., that the disappearance of mentality indicates that the various tendencies of the organism have become adjusted to one another. In any case, we can recognize the circular mode of procedure here, viz., judging the absence of mentality by the perfect adjustment and the perfection of the adjustment by the disappearance of mentality.

Finally the definition of intelligence given in the summary of the article as 'the process by which various stimulated tendencies of the organism are adjusted to the execution of a series of movements resulting in adaptation to the environment' must evoke a shrug of the shoulders on the part of the critical reader. Is a tendency of the organism wholly disparate from mentality? And does mentality always lead to adaptation? Did Socrates and Giordano Bruno adapt themselves to the environment, or shall we say perhaps that they lacked mentality? What, behavioristically, is the difference in adaptation between the martyr who willingly goes to the stake rather than renounce his principle, the daredevil who loses his life as a result of some foolhardy stunt, and the prehistoric animal that failed to survive the hardships confronted with in the struggle for existence? Lastly, just how,

physiologically or psychologically, does the stimulated tendency become adjusted to the execution of movements? Is there not a *hiatus* in thought here that needs clearing up?

Turning from the cruder to the more critical type of behaviorism as represented by Perry, we notice the endeavor to overcome the difficulties already outlined. "The important feature of docility is not adaptation to the environment, but the acquiring of specific modes of adaptation, and performance determined by the experience of adaptation."¹ Of greater significance is his warning held out to orthodox behaviorism against 'any hasty or contemptuous dismissal of the traditional association of purpose with non-physical or "ideal" entities'; and the 'danger of confining our analysis too closely to the lower forms of mind,' is explained by the fact that "most human purposes deal with 'objects' of hope, fear, or aspiration that find no place at all in the field of nature as that is defined by the physical sciences."²

Perry's cautious procedure in his later articles as compared with his more zealous position a decade before, when he defined mind as 'behavior or conduct together with the objects which these employ and isolate' or as 'organization possessing as distinguishable but complementary aspects, interest, body, and objects,'³ should make us alive to a real departure in the direction of moderation evinced by the more circumspect representatives of the behaviorist movement.

When we stop to make the balance sheet in order to learn in what way behaviorism has furthered our knowledge in regard to the problem of intelligence, not merely learning, but grasping, discriminating, judging, interpreting, we come face to face with a minus quantity. The very substitutes in terminology that behaviorism has introduced are highly unsatisfactory. (Incidentally it strikes me that Perry's word *docility* is not a very felicitous designation for the capacity to learn, since docility bears a somewhat contemptuous

¹ R. B. Perry, 'Docility and Purposiveness,' *Psych. Rev.*, 1918, 25, 19.

² *Loc. cit.*, p. 7.

³ R. B. Perry, 'The Mind Within and the Mind Without,' *J. Phil., Psych. & Sc. Methods*, 1909, 7, 175. The term interest is employed in its biological sense.

connotation. The docile man is generally submissive and suggestible, lacking in initiative. It is clear, however, that Perry's meaning of the word is otherwise.) What boots it to know that expectation, desire, understanding or inference is probably a set of incipient adjustments, so long as we are not provided with the differentia? With the mental state, expectation or desire we are intimately acquainted. The incipient adjustments that are to replace as scientific material the mental states of which we have direct knowledge are not only hypothetical and speculative but vague and general. The hypothesis in the physical sciences is at least clearly formulated and, mathematically applied, it actually explains a number of phenomena by showing how one principle governs them all. The behavioristic hypothesis or assumption is a stagnant affair in that it usually promises a great deal and balks at the crucial point. We expect a mental experience to be translated into behavioristic components and are disappointed on learning that the experience reduces to the phrase 'a set of adjustments.' Are we to discard the analysis or our experiences for the sake of arriving at colorless conjectures or at commonplaces decked out in biological language?

THE MEANING OF THE TERM 'PRACTICE'

BY GEORGINA STICKLAND GATES

Barnard College, Columbia University

A question which often confronts a student interested in the learning process, is concerned with the meaning of the term 'practice' in the literature of psychology. Should this word be taken to mean repetition with or without observable improvement of the function, or does it connote something more than this, repetition with observable gain in quantity or quality of the product as a result? Or does the term refer not primarily to any fact of behavior, but to the conscious processes which accompany repetition?

The problem is not necessarily a futile one. It is important, first, in view of the need for definition of any term used in science. To determine the reliability of our terms is surely as important as to determine the reliability of our averages or measures of variability. A word should be no less exact than a coefficient of correlation. The word practice is certainly as essential (at least for some schools of psychology) as the terms subconscious, levels of attention, redintegration, transfer, apperception, innervation, imitation, libido. Our understanding of many of the important psychological discussions may be dependent, moreover, upon our concept of the term. If we use the word practice to refer to an experiment on transfer of training, it is necessary that we observe whether mere repetition or repetition accompanied by improvement takes place, not only in the test, but in the training series. Discussions concerning the effect of practice on ability to see through an illusion, on suggestibility, on ability to differentiate tones, to grasp small visual details, are more or less meaningful as the term is taken in one or another sense. In the field of individual differences, it is of greatest importance that we distinguish between repetition

and its usual results. We can present no general theories concerning the influence of practice on, let us say, correlation between tests, until we decide whether practice may have no observable effect, or always has some observable effect upon individual scores.

In order to discover the meaning of the term, one turns first to the text-books in common use, and then to the original reports concerned with the 'effects of practice,' 'practice curves,' etc. The quotations which follow are taken as samples of possible view-points. They are not inclusive—on the contrary a random selection has been made from the works of accessible representative authors.

The indexes of the text-books of James, Sanford, Angell, Pillsbury, Bagley, Calkins and Breese do not contain the word 'practice.' These authors instead discuss learning, habit formation or training.

But in Meyer's translation of Ebbinghaus¹ there is a definition:

The word practice refers to a number of different phenomena having this in common, that they occur when the same mental function is frequently repeated, either in immediate succession or with moderately long intermissions. To a large extent, practice is identical with the selective and supplementing functions of the mind which are discussed above. But certain effects included in the term practice cannot be understood thus and must be regarded as the signs of a fundamental law of the mind.

Practice, therefore, is a general term referring to the wonderful adaptation of mind to the external world for the purpose of self-preservation. By association and reproduction, mind adapts itself to frequently recurring events and anticipates them. By practice, it adapts itself to those events which occur with particular frequency and which are of particular importance. These events are through practice comprehended more delicately, more quickly and more inclusively. They are responded to in a manner tested as the most fitting and the most prompt, and yet requiring only a minimum of mental energy, of which more than a limited amount is at no time available. Without having to neglect the ordinary and as

¹ 'Psychology—An Elementary Text Book,' by Hermann Ebbinghaus; translated and edited by Max Meyer, 99 f., 101 f.

such important, mind has energy left to devote to that which is new, unusual, surprising.

Ebbinghaus refers evidently to the usual effects of repetition. Whether repetition, if fairly frequent, will always produce these phenomena, he does not say. We have no way of knowing what would be Ebbinghaus's comment upon an observation of a repetition with no apparent effects. The results described, he apparently assumes to be universal.

Kulpe¹ also makes repetition and improvement identical. Practice seems to be an active force, unknown but with measurable results.

We shall not mean by practice a special psychical capacity, or even a new mental act, but merely a number of processes, not very exactly known, whose effect is to facilitate an operation which has been frequently repeated.

Practice and Fatigue—We mean by these terms processes which are ordinarily alike dependent upon the number of observations, but which exert diametrically opposite influences upon sensitivity and sensible discrimination. To practice is due a steady increase in delicacy of perception and readiness of judgment, to fatigue a steady decrease in both. The words are so familiar and intelligible that it is unnecessary to define them. . . . Practice expresses itself psychologically (1) in an increase of attentional concentration, with all its accompanying advantages, and (2) in an increased capacity of reproduction.

Titchener's² definition is in terms of consciousness. We are not sure whether this consciousness is achieved as a result of repetition or whether (as seems more probable) it is the consciousness that accompanies repetition with desire to improve.

Practice is an integrative, as distinguished from a discursive consciousness; the focal processes are few in number, extremely clear, and protected by the negative effect of the determination against interference from casual associations. The effects of practice, if we take that phrase in its widest sense, are manifold. Thus it was found in experiments upon the discrimination of lifted weights

¹ Kulpe, 'Outlines of Psychology,' 7, 42 f.

² Titchener, 'A Text Book of Psychology,' 538 f.

that the influence of practice shows itself in no less than five different ways. It makes the observers physically stronger—and a change in physical strength may in this case mean a change in the absolute impression of the stimuli; it makes the lifting more uniform; it raises the level of attention; it increases the likelihood of judgment by absolute impression; and it may shift the observers' standard of comparison so that a difference of stimuli which finds expression in 'heavier' or 'lighter' at one stage of the experiment may be recorded in the words, 'much heavier,' 'clearly lighter,' at a later state. All these effects are closely related; but their number and variety prove that the general statement of the text 'practice favors discrimination' covers a large number of coöperating factors.

The practiced consciousness is characterized on the functional side by a maximum degree and constant direction of attention, with minimum effort (secondary passive attention), ease of discrimination (delicacy of perception) and a high degree of capacity for reproduction, implying great readiness and confidence of judgment.

The meaning of the phrase 'effects of practice' is not quite clear in view of the definition given in the first sentence. Is the increase in physical strength, the more uniform lifting of the weights, due to this peculiar kind of consciousness *per se*—or must there be repetition of the process of lifting weights, accompanied by an integrative consciousness? Is a repetition which includes measurable improvement in the discrimination ability essential for an improvement in level of attention? Would a person who reached a limit beyond which no further improvement was observable still be practicing? Such a subject might have an integrative consciousness. But none of the observable 'effects of practice' would appear. Granted that from Titchener's point of view the integrative consciousness is essential for practice, how many more factors, if any, must we add? Is the practiced consciousness the result of the mere existence of the practicing consciousness, or of the exercise of this consciousness, or of this exercise with "visible effect upon the external world?"

In Myers's Text Book¹ there is no definition of practice but excerpts will give the meaning.

¹ Myers, 'Text Book of Experimental Psychology,' 1, 61; 2, 292, 180.

It is not difficult to devise experiments which shall test the degree of practice in speed and accuracy of movement. [Experiments suggested.] By such means the effects of increasing practice, fatigue and the extent of retention of practice can be qualitatively determined.

When Myers speaks of 'degree of practice,' 'extent of retention of practice,' he evidently refers not to mere repetition (for degree of repetition, extent of retention of repetition are meaningless) but to repetition, plus some observable effects—to repetition accompanied by improvement.

Other references are less clear. Speaking of what occurs as a result of repetition of an illusion, he says: "Many, at least, of these illusions tend to disappear with continued practice, even though the subject remain ignorant throughout of the nature of the illusion. In the Müller-Lyer illusion, it has been shown that these effects of practice only occur when the exposure of the figure is prolonged. During momentary exposure the subject has no opportunity of learning to disregard the end lines and to limit his attention to the horizontal line, the length of which is being estimated." Is the repetition of the momentary exposure practice? Not in Myers's other use of the term. The phrase 'effects of practice' seems awkward, if the word means the same as it did in the phrase 'extent of retention of practice.' The simplest meaning in the second quotation would seem to be mere repetition.

In a discussion of mental work and practice we find these statements: "Along with increasing fatigue goes increasing practice"; and, farther down the page, "The influence of practice is not evident at the early stages of experience." In one sense repetition plus its effects is evidently meant. In the second we must substitute repetition.

Wundt¹ implies that practice and improvement with repetition are not identical, since this improvement is the universal 'result' of practice. Then is practice mere repetition? If so, how can we believe that it has this 'universal result'?

¹ Wundt, 'Outlines of Psychology' (translation by Judd), 233.

These physical concomitants are the effects which practice produces on all organs, especially on the organs of the nervous system. As a universal result of practice we observe a facilitation of action which renders a repetition of the process easier. To be sure we do not know any details in regard to the changes which are effected in the structure of the nervous elements through practice, but we can represent them to ourselves through very natural analogies with mechanical processes, such, for example, as the reduction of friction resulting from the rubbing of two surfaces against each other.

Whitley¹ speaks frequently of effects of practice and changes with practice. Her real concept of the word seems, however, to be improvement by repetition. She refers to the lifted weight test: "After a certain date, too, each subject was told what the real weight was, in the hope of facilitating practice by this means." And, in her conclusion: "On the whole higher mental functions are sooner susceptible to practice than are sensory functions, the more so again if they are novel."

Wells² uses the word in both senses, though his meaning is fairly clear. He says: "We have then, finally (1) a difference in the individual's (resp. functions) fundamental plasticity, i.e., ability to profit by practice; (2) a difference in the actual amount of practice experienced; and (3) constitutional factors, independent of plasticity, in the nervous system." Here "ability to profit by repetition" is obviously meant. "Amount of practice experienced" probably refers to amount of repetition of the process. Whether these repetitions must be accompanied by improvement, Wells does not state. Yet the distinction would seem to be an important one. Does repetition after observable gain has ceased, have any influence on individual differences?

Again, we find: "A far-reaching sex-difference in susceptibility to practice seems, so far as concerns the functions covered by these experiments, quite improbable." Does he

¹ Whitley, 'An Empirical Study of Certain Tests for Individual Differences,' 115, 138.

² Wells, 'The Relation of Practice to Individual Differences,' *Amer. J. of Psychol.*, 25.

mean susceptibility to repetition? Then susceptibility would only be a very poor word for opportunity. What Wells does mean is susceptibility to improvement, through repetition. Such a usage differs from the one quoted in the last paragraph.

Hollingworth¹ says: "By final capacity is here meant a degree of ability which, having been attained as the result of constant practice, remains practically unchanged by further practice during, say, 100 later trials."

But in another article we find: "With practice, then, the average correlations of all tests become positive and the coefficients become greater the longer the practice is continued."

If we take practice in the first sense in which it is used—the second quoted statement is indeed a remarkable one. Repetition of tests, necessitating, perhaps, no change in the individual scores (see first quotation) would, nevertheless, increase correlations between the tests.

Book² avoids the difficulty by referring usually to 'phenomena influencing learning,' 'learning curves,' etc. His mention, however, of 'stages of practice' implies a wider meaning given to the word than mere repetition.

Besides this use of the term practice to designate indiscriminately mere repetition or repetition with improvement, and its use to mean a certain kind of conscious process assumed to accompany the repetition of an act, we have a third and much more definite meaning attributed to 'practice.'

The following quotations are taken from Whipple.³

The consensus of opinion is that at least in comparison with many other mental activities the discrimination of lifted weights is but little affected by practice.

This practice effect is, however, rapidly lost, being reduced very definitely within eight days and completely lost within a month. (Esthesiometric Index.)

¹ Hollingworth, 'Individual Differences Before, After and During Practice,' *Psychol. Rev.*, 1914.

Hollingworth, 'Correlation of Abilities as Affected by Practice,' *J. of Educ. Psychol.*, 1912.

² Book, 'The Psychology of Skill.'

³ Whipple, 'Manual of Mental and Physical Tests,' 228, 255, 273, 601, 470.

Practice has a curiously small effect upon the range of attention when once the period of preliminary habituation to the arrangement of apparatus and method is passed.

Practice has very little effect upon the suggestibility of S's; at least Binet found that, when twelve older children (16 years) repeated the test by the first method five times in immediate succession, there was no alteration in the average number of times that suggestions appeared.

Practice increases the speed of computation, even when the test demands the exercise of associative connections in which S has already had extended school-training.

Evidently Whipple means by practice pure repetition, since he uses the term in cases where no improvement is apparent.

Yerkes¹ speaks of a law of temporal distribution of practice. "Brief intervals of practice,—repetition for memorizing are more valuable than long-continued practice." This reference is again to the process of repeating—not to its psychical accompaniments or visible effects.

Thorndike² differentiates improvement and practice. A number of quotations are needed to suggest his meaning.

Education and applied psychology generally consider mental functions, especially under the concept of efficiency, as improving or deteriorating. They then should describe a function as a number N of amounts ($a, b, c \dots n$) each of some one thing—quality, characteristics or feature, which varies only in amount or degree.

The scale by which the amount or degree of one of these component features of a mental function is measured is very often a graded series of external products, or features of products produced by the function's activity. . . .

Any intelligible statement about a function's efficiency is then, as a rule, a statement defined as vague, precise or rough, about some score or combination of scores.

He heads a chapter, 'The improvement of mental functions by practice,' and, in the opening paragraph—

This chapter and the six which follow are to present the facts

¹ Yerkes, 'Introduction to Psychology,' 308 f.

² Thorndike, 'Educational Psychology,' 2, 83 f.

known and problems pending concerning the amount and limits of improvement of mental functions with practice, the rate of improvement, and any changes in it as practice proceeds, the factors which constitute the improvement, the circumstances which condition it, and the effect which the improvement of any one mental function has upon others.

Had Thorndike said—in the last phrase—"the practice of any one mental function"—who could have been sure what he meant?

Another quotation presents the differentiation even more clearly. In speaking of the fact that most men remain far below their limit of efficiency, Thorndike says:

First, hardly any functions have ever been practiced in the course of the scientific study of mental functions, which did not improve. . . .

Second, there are striking cases of individuals who have had enormously long practice, as taken in the course of schools or trades, and who have kept at the same level of efficiency for a long time, but who, under more favorable conditions, make notable advances.

Culler¹ similarly speaks of 'ability to profit by practice,' 'susceptibility to improvement by practice,' 'practice effect,' 'errors committed in practice,' etc. Bair² seems to mean mere repetition when he speaks of "continuing for twenty, thirty, and forty practices in one order before the other one was practiced," or "by practicing a particular reaction, or series of reactions, until the new order becomes automatic," or "the time required from day to day is constantly reduced by practice until the physiological limit is reached."

Bryan and Harter³ use the word in this same meaning. They say: "A fact which seems to be highly significant is that years of daily practice in receiving at ordinary rates will not bring a man to his own maximum ability to receive. That daily practice in receiving will not insure improvement,"

¹ Culler, 'Interference and Adaptability.'

² Bair, 'The Practice Curve,' 37, 38, 39.

³ Bryan and Harter, 'Studies in the Physiology and Psychology of the Telegraphic Language.'

etc. They speak of stages of progress where others might refer to stages of practice.

We find then, at least four fairly distinguishable meanings. Practice may mean (1) phenomena or processes which occur in the individual when an act is repeated, (2) it may refer to a certain kind of consciousness, (3) it may mean the act of repeating when repetition is accompanied by gain in efficiency, or (4) it may mean mere repetition whether or not accompanied by improvement. That it is difficult to decide which meaning an author has in mind is evident, not only from the statements with contradictory usages quoted here, but from the fact that it is very easy to disagree with the present writer's interpretation of an author's meaning.

Which of these different meanings shall the student of practice adopt? Shall he define practice in terms of consciousness, or of external events? Shall its effects be emphasized or the activity itself? What do words like practice curve, practice limit, practice level, susceptibility to practice, effects of practice, facilitating practice, stages of practice, practiceability, extent of retention of practice, degree of practice, mean? In a concrete case, how can one determine whether an individual is or is not practicing? Must we observe merely whether he repeats an act, whether he improves, whether certain processes accompany the repetition, or, perhaps, whether his consciousness is or is not integrative?

A few suggestions might be offered. The difficulties involved in distinguishing between the consciousness of the beginner and of the expert are obvious. A given individual is often unable to tell whether he is practicing or playing the piano. When the object of our description is a turtle perfecting the learning of a maze, opportunities for discussion and reconstructed definition are endless.

It seems better simply to stand aside and observe as much of the activity of the individual as it is possible to see and the product of his performance. Then we may say that the expert is or is not practicing as we take repetition or repetition with gain as our standard. If we choose repetition with measurable improvement, it still involves us in diffi-

culties. Suppose we had used some other method of scoring, a different measure of 'product produced per unit of time.' What seemed a level, might then appear to be an ascent. Suppose we had tested two 'experts' after years in which neither had had 'practice.' The one who had made the most repetitions—though these repetitions showed no gain at the time, might be shown to have retained the process better. If, moreover, we take the word improvement to mean immediate, observable change in score, we must not only dismiss the expert's work from our discussion, but exclude also such work as that of Bryan and Harter's subjects who repeated the work of receiving, but who for a long time made no apparent improvement, or of Book's typewriting subjects who were likewise on an improvement level. Because anything but immediate improvement is difficult to measure and likely to prove ambiguous, and because if we confine ourselves to the meaning—immediate gain, we must exclude so many activities called both commonly and in scientific literature, studies of practice, it seems best to abandon improvement as a necessary criterion of practice.

If we take the word in its widest sense—it means mere repetition of any process or exercise of any function. Then responding to one's name is practice in such a reaction, putting on one's shoe is practice in that process, the grocer who has footed up bills for thirty years is still practicing, the expert typist and the expert color-namer are practicing, whether or not they experience any measurable effects of this practice. The use of the word in this sense enables one to include under the subject practice—such activities as those of Whipple's subjects who repeated tests of visual apprehension without, in his opinion, appreciable gain, of Hollingworth's subjects who continued repeating tests after an improvement limit had apparently been reached, of individuals working at one level of efficiency for a long time with, however, subsequent gain, as was the case in the experiments of Book, Bryan and Harter, and others. Such a usage also avoids theoretical difficulties implied in other ways of employing the term. We do not assume any invariable effects

of practice or influence of practice. We have before us the observable event—some one repeating something. Then, we may inquire, what is the effect of this repetition on the product produced, on the person himself, or on his similarity to or difference from other individuals.

This definition would seem to make necessary a departure from the customary phrases built upon the word practice. If practice is to mean mere repetition then we can no longer talk of stages or degrees of practice, of susceptibility to practice, of practiceability, of facilitating practice,—or even perhaps of such time-honored concepts as practice curves, practice limits or practice levels. We may easily substitute stages of learning or improvement for stages of practice, susceptibility to improvement by practice or ability to learn by practice for the second and third phrase. We may talk of facilitating learning or improvement. It is more difficult and perhaps unnecessary to give up practice curve. Yet a practice curve in the sense of a curve representing repetitions would only be, if repetitions were evenly distributed, a straight line. The phrases 'learning curve' or 'curve of improvement' (stating as they do just what the curve is intended to represent), are perhaps preferable. For practice limit and practice level again we might use limit of improvement or learning.

CONSCIOUS ANALYSIS IN LEARNING

BY H. R. CROSLAND

University of Oregon

Doctor F. A. C. Perrin has recently presented a paper on 'Conscious Analysis *versus* Habit Hierarchies in the Learning Process'¹ which is based on conclusions reached by himself after testing over long periods of time the learning capacities of 100 subjects in three sets of performance tasks. The Bogardus fatigue test, a set of balancing tests, and a coördination (groove, maze-tracing) test comprised the three series. The 100 reagents were 'untrained' in the technique of introspection which was employed. The paper quotes no introspections, tabulates no quantitative data, and presents no graphic representations of objective results. The author undertakes to criticize certain prevailing and historical concepts, and then presents his own hypothesis of habit hierarchies.

Perrin contends² that "one reads the . . . literature in vain for satisfactory answers to" such questions as: (1) Does consciousness manifest itself in the initial stages of a learning process? (2) If it is present, is this consciousness ideational and cognitive rather than emotional in nature? (3) If it is emotional, does it facilitate or impede the learning process? (4) If this consciousness is cognitive, is it a particularized consciousness which is absent from non-learning situations? (5) Does this initial consciousness select correct reactions and eliminate from the learner's *repertoire* of reactions the incorrect? And (6) is this consciousness aroused in the face of new, or difficult, or complex situations?

At first glance these questions would seem to comprise no small assignment and to be not easy of solution, especially

¹ *J. of Compar. Psychol.*, 1921, 1, 287-308 (a short review of which is to be found in the *Bulletin*).

² *Op. cit.*, p. 288.

when our author undertakes to answer them, and to answer them more satisfactorily than was formerly done, in the compass of a paper of only twenty-two pages. The assignment bulks even larger if one has no tables of results, no curves, and no protocols of introspections by means of which one can be guided to the author's own conclusions and interpretations.

The unsatisfactory nature of an hypothesis, we believe, will be found to consist of any one or of any combination of the following characteristics: First, the hypothesis may be quite true to facts, but may concern itself with facts which have no practical bearing upon life's everyday problems. Second, the hypothesis may be almost entirely a rational, or speculative, or metaphysical, or logically closed-system concept, the facts in this case being not empirically ascertainable. Third, the hypothesis contradicts the known facts. Fourth, the hypothesis is founded upon only a few out of a great many facts, the fallacy being that of reasoning from the part to the whole. Fifth, the hypothesis may be just a manner of interpreting known facts, and may not preclude other interpretations which may or may not better jibe with a much larger body of facts and other hypotheses. We do not pretend that this list of inadequacies of hypotheses exhausts the possibilities, but we give this list in order to attempt to discover just what inadequacies Perrin will iron out in the giving to us of his own more adequate concepts. The pure scientist, of course, is not perturbed by the first inadequacy in our list (and, parenthetically it can be said, if the hypothesis that consciousness is a causal agent fits this category, the behaviorist, engaging in 'pure' science, has to seek other grounds for his criticism of this hypothesis). The experimental scientist, and in the current usage of the term 'scientist' he is usually meant, is not quite satisfied with hypotheses of type two, and he demands the empirical justification of a concept or an hypothesis; but, if he is fair, it seems as though he must admit the tentative and pragmatic nature of all hypotheses, even those which seem to rest upon

the most firm and secure empirical foundation. No one, we suppose, would defend a concept which obviously contradicts facts, nor would we lose our friendship for those persons who attack such a faulty concept, however vigorously the attack is made. But, unfortunately, difficulties arise and multiply themselves when concepts must be measured by standards which we included under rubrics four and five above. We suppose that the fallacy of begging-the-question will somehow find itself in company with other fallacies in these two groups. In relation to these five types of the inadequacies of hypotheses, we believe that the reader has a right to expect of an experimentalist that he, in experimentally substituting adequate concepts for what he conceives to be unsatisfactory hypotheses, establish his newer hypotheses and convince one that they are at least as satisfactory as, if not more satisfactory than, the concepts which he attacks.

It has been asserted frequently that the doctrine of the 'subjectivity' of consciousness, along with the introspective method, was really the producer of results which caused so much divergence of opinion and lack of agreement among the older psychologists. If this statement were true and were wholly true, one could quite reasonably expect, on one hand, that behaviorists agree among themselves, and on the other hand, that objectivists themselves agree. But, on the contrary, we find that such behaviorists as Watson,¹ Yerkes,² Thorndike,³ Franz,⁴ and Peterson⁵ do not agree. And, if what Kantor⁶ has called "esoteric introspection" has been

¹ 'Psychology from the Standpoint of a Behaviorist,' Philadelphia, J. B. Lippincott & Co., 1919.

² (1) 'The Mental Life of Monkeys and Apes, etc.,' *Behav. Monog.*, 1916, 3, (No. 12), 1-145; (2) 'Behaviorism and Genetic Psychology' (a review of the second edition of Hobhouse's 'Mind in Evolution'), *J. of Phil., Psychol.*, etc., 1917, 14, 154-160.

³ 'Animal Intelligence' (mentioned by Yerkes in his review of the second edition of Hobhouse's 'Mind in Evolution,' *op. cit.*), N. Y., The Macmillan Co., 1911.

⁴ 'Cerebral-Mental Relations,' *PSYCHOL. REV.*, 1921, 28, 81-95.

⁵ (1) 'Completeness of Response as an Explanation Principle in Learning,' *PSYCHOL. REV.*, 1916, 23, 153-162; (2) 'Frequency and Recency Factors in Maze Learning in White Rats,' *J. of Animal Behav.*, 1917, 7, 338-364; (3) 'The Functioning of Ideas in Social Groups,' *PSYCHOL. REV.*, 1918, 25, 214-226.

⁶ 'Suggestions toward a Scientific Interpretation of Perception,' *PSYCHOL. REV.*, 1920, 27, 212.

the great obstacle in the path of the harmonizing of experimental results and has resulted in disagreements among the older users of the method, why, we ask, do mental-testing objectivists fail to agree?¹

We shall proceed to ascertain if Perrin satisfactorily answers the queries with which he opens his paper. He reports, in the main, conclusions reached by himself from the results obtained in three, of seventeen, motor coördination, learning tests. The results themselves, that is, a fair sampling of quoted introspections, and of objective error and time results, he nowhere gives in his paper. Inasmuch as his paper is avowedly an experimental one, and not merely a discussion or a critical review, serious objections can be raised to his method of substituting satisfactory hypotheses for unsatisfactory explanations. The tests were done with one hundred subjects, most of whom, if not all, were 'untrained' in the introspective technique and, judging from what little description is given of the introspections in Perrin's paper, not one of his observers had any conception of the necessity of an introspection's containing descriptions of the functional operations of a mental content besides merely cataloguing this or that structural phenomenon;²

¹ There never was greater disagreement among objective mental testers than at the present time, despite their full measure of 'objectiveness.' Witness their disagreements concerning the constancy of the 'I. Q.,' the variations in test scores by reason of the past experiences of the subjects, the age at which maximum mental acuteness is attained, the shape of the curve of the growth of intelligence, the linear or non-linear form of intellectual growth, and the qualitative differences discovered in mentalities of measured equality. See Doll's study, *Psychol. Monog.*, 1921, (No. 131), 130 pp; a symposium on 'Intelligence and Its Measurement' in the *J. of Educ. Psychol.*, 1921, 12, 123-144, 195-212, and 271-275; discussions by Peterson and Freeman, *ibid.*, 148-154 and 155-158; discussions by Rugg, Colloton, Poull, and Terman, *ibid.*, 315-341, 401-407, and by Roback, *ibid.*, 439-444. See also Root's study, *Psychol. Monog.*, 1921, (No. 133), 134 pp.

² For functional descriptions of mental structural phenomena, see these studies: (1) Woods, E. L., 'An Experimental Analysis of the Process of Recognizing,' *Amer. J. of Psychol.*, 1915, 26, 313-387. (2) Fisher, S. C., 'The Process of Generalizing Abstraction, etc.,' *Psychol. Monog.*, 1916, 21, (No. 2), v-213 pp. (3) Fisher, S. C., 'An Analysis of a Phase of the Process of Classifying,' *Amer. J. of Psychol.*, 1917, 28, 57-116. (4) Wheeler, R. H., 'An Experimental Investigation of the Process of Choosing,' *U. of O. Publications*, 1920, 1, No. 2, 59 pp. (5) Fernberger, S. W., 'An Intro-

apparently, the introspections contained merely structural data. In this connection, it ought to be said, introspection, as an experimental method, has passed through several stages of development and refinement, from the merely *Kundgabe* type, through the structural and structural-meaning type, to the functionally analytical type. And it would be quite a safe assertion to state that naïve and 'untrained' subjects cannot have attained to much more than introspection of the *Kundgabe* type.

The reader by now will have judged that Perrin, in using the introspective method, must have had some faith in it and in its results, even though it was used with naïve subjects. This using of the introspective method, it seems to the writer, is not consistent with the main point of Perrin's paper, that consciousness is an ineffective factor in learning, regardless of the fact that consciousness need not be regarded as an entity in itself but may be regarded as a criterion as to what integrations are being effected at the moment in the cortex (as Warren¹ has valiantly tried to show).

His initial queries Perrin answers tentatively in this manner: (1) The alleged heightened initial consciousness of the human adult learner has been over-stressed. (2) This consciousness has erroneously been called cognitive instead of emotional. (3) Conscious analysis follows, rather than precedes, the various acts of learning. (4) Conscious analysis by no means follows consistently the retention and selection of new and difficult coördinations. And (5) "reasons exist for" asserting that the learning process "is necessarily an unconscious process."

Two reasons, referred to in answer number five, just quoted, are: (1) Certain difficult motor coördinations are acquired which baffle all attempts at analysis by the introspective Analysis of the Process of Comparing,' *Psychol. Monog.*, 1919, (No. 117), 161 pp. (6) Snoddy, G. S., 'An Experimental Analysis of a Case of Trial and Error Learning, etc.,' *Psychol. Monog.*, 1920, (No. 124), 78 pp. (7) Crosland, H. R., 'A Qualitative Analysis of the Process of Forgetting,' *Psychol. Monog.*, 1921, (No. 130), 159 pp.

¹ 'Human Psychology,' Boston, Houghton Mifflin Co., 1919; and 'Psychology and the Central Nervous System,' *Psychol. Rev.*, 1921, 28, 338-364.

spective reagent (and we wonder if they are not just as baffling to the extreme behaviorist). And (2) coördinations are selected and retained unconsciously, that is, with no memory of the original and initial occurrence and with no conscious plan of performance on the observer's part (and be it noted, we do not teach students to live their lives without conscious purposes). Here the writer supposes that, *logically*, the proposition involved would read: "Some X is not-Y, therefore all X is not-Y, or is not Y," or "Some X is not-Y, therefore some Y never was and never can be some other part of X."

The author vigorously attacks the doctrine that an anticipatory image, or conscious set of plans, precedes the sequences of acts in learning. But he has very little to say of perception itself as a control, of sequences of perceptions as controls, and of changes in a given perception itself as controls. It can be argued that these omissions are serious, inasmuch as all of his tests required much perceiving, demanded many and varied sequences of perceptions, and involved a great multiplicity of changes in even one single perception. For instance, few persons will deny that visual perception, or the visually perceived situation, is of great importance for the coming of compensatory movements in maintaining one's balance while whirling on one's heel, in the causing of giddiness when one descends in an elevator, and in vertigo caused by watching spirals in the Masson disc experiment. Snoddy¹ has also treated at length the lack of images in the reagent's tracing of his metal star; but the question of control by visual perception seems to have offered surprisingly few problems which crave for solution, if one is to judge by the ready fashion in which he uses the 'analytical receptor' concept; and, moreover, in his experiment particularly, as in certain of Perrin's experiments, the very conditions of experimentation were decidedly of such a nature as to prevent the use of various types of imagery.

One of Perrin's favorite arguments is the fact that he discovered no positive correlation between a subject's ability

¹ *Op. cit.*

to introspect and his ability to make progressive improvement in the various tests. The subject cannot introspect, but he can learn quickly and well the task set for him in the performance test; therefore, consciousness, the phenomenon to have been introspected upon, had nothing to do with the learning. To make this argument look even more absurd, suppose we consider the students who constitute our first-year, elementary psychology classes, most of whom come into our classes believing that the objects which they see, hear, touch, etc., must really exist in space as they seem to exist. This object-mindedness is ingrained in them, doubtless by their past experiences and by their native equipment, and these students are the very students who will find introspecting a well-nigh impossible task, especially before perception and illusion have been demonstrated and discussed in class and before practice in introspection has been given over a long period of time. Are we to measure the intelligence of these students or their learning capacities by their object-mindedness, by their inability to introspect, even before they have been taught to introspect, if it is at all possible to teach them to do it successfully?¹ Frankly, the writer knows of no instructor or writer in psychology who has been characterized by such obtuseness, or has been engaged in such

¹ And one need not regard this necessary period of preliminary training a 'holy vigil' before or a 'secret initiation' into the occult! We suppose that all scientists will attest that a naïve and untrained person cannot, with fair accuracy, use a microscope in bacteriology and histology; and that, after much training has been had, the users of the microscope will show great individual differences, there resulting poor, mediocre, good, and skillful observers. And, to go farther, one can quite rightly assert that objective observation (perception) is as liable to error and illusion as is introspective observation. Perception, at best, is always relatively illusory in nature; and to say that we saw this or that object, or this or that movement on the part of another person, does not vouchsafe any greater degree of accuracy than to say that we had such and such a visual image of such and such an object. We should like to emphasize this point, for we hear much of objective methods of observation and the accuracy there obtained. But in all objective observation, the observing is accomplished through means of sense-perception, and the sources of error and illusion in perception are legion! And, with regard to the effects of training on introspectors, it can be stated that certain observers will never, in a whole lifetime, get beyond the *Kundgabe* stage! The writer certainly does not crave the responsibility of attempting to train to introspect the person who has no visual imagery with which to begin and whose analyses could best begin on visual imagery.

arbitrary intelligence-testing, or has made such gratuitous use of the introspective method. If this type of argument is aimed at other studies, in which consciousness has been found to have been effective, the writer believes that our author is fighting a straw man or a ghost of his own construction, for, as far as we know, no champion of the introspective method was ever so courageous.

Perrin finally takes up the various theories of the adoption by the reagent of correct reactions and the elimination of the erroneous responses. That the final act, in a series of acts, is the correct one, and will, by reason of its *recency*, be selected in the next series of trials, he discards as a working hypothesis, because he found that successful acts occurred in the middles of his series of trials and not necessarily at the concluding ends. Also finding difficulty in conceiving of forty-three glandular changes per minute, to correspond with the forty-three trials per minute, in the Bogardus test, he discards the *affective* (glandular) theory (as though any one to date had successfully demonstrated that the affections are dependent on or are identical with glandular secretions!). A third theory, namely, that, after a successful act has been discovered by the reagent, he then *consciously observes and adopts* the elements or methods in the successful act, Perrin likewise discards, on two grounds, namely (1) at times both acquisition and fixation were unconscious, and (2) in one of the tests the observer was prevented, by the nature of the experiment, from attending to the various steps or sequences of acts by means of which he arrived at his successful result. A fourth hypothesis, that the correct act, by the very nature of the experiment, must be the act most *frequently* consummated, is rated highly by the author, because of its simplicity, its harmonious relation with neurological hypotheses, and its drawing no distinction between acquisition and fixation; but he asserts that his observers were under no physical compulsion (as usually is true of the technique of maze experiments) to act correctly, to place blocks correctly in the Bogardus test. To bolster the doctrine of *frequency*, he introduces his older doctrine of *habit hierarchies*. The matter finally resolves

itself into a matter of systems of habits, some general and some specific, always set off by stimuli, for the most part peripheral stimuli. And, to the writer, seemingly all semblance of central control, except as a habit must have a base of organization somewhere in the central nervous system, is gone and has been removed on so few data,—these unpublished,—and with what seems such an easy and casual manner of treating a host of really difficult problems.

Yet, these habit hierarchies are supposed to belong to different orders, some high and some low. One is inclined to wonder what can be the real basis of *high* and *low* orders of habits, if the basis is not one of generalness and particularity solely,—if it is not one of levels in the cord and brain,—if it is not one of the complexity and the simplicity of the musculature involved,—if it is not one of the presence or the absence of consciousness indicating the presence or absence of certain cortical integrations? The writer has before him at the moment Perrin's doctorate monograph,¹ entitled 'An Experimental and Introspective Study of the Human Learning Process in the Maze,' in which is set forth a far more clear and comprehensive analysis of the development of different orders of habits through the mediation of consciousness than one can find in the paper which we have undertaken to discuss. It seems to the writer quite unfortunate that this later and shorter paper should have diverged so greatly from the results obtained in the earlier and far more systematic and rigorous investigation which the writer regards as an exceedingly creditable piece of work.

To pass on, Perrin regards thoughts as almost solely matters of language habits, following Watson's famous *dictum*,² even in the face of such very pertinent criticisms as those presented by Otis³ and Calkins.⁴ This is enough to convince us that Perrin is attempting to have the behavioristic

¹ *Psychol. Monog.*, 1914, 16, (No. 4), 97 pp.

² *Op. cit.*, p. 322 ff.

³ 'Do We Think in Words?' *Psychol. Rev.*, 1920, 27, 399-419.

⁴ 'The Truly Psychological Behaviorism,' *Psychol. Rev.*, 1921, 28, 1-18.

Since our paper was written we have discovered in the *Brit. J. of Psychol.*, Oct., 1920, a symposium in which Watson's *dictum* is discussed in detail.

horse pull his wagon; and that some of his arguments are even less satisfactory and convincing than those criticized by Yerkes, Franz, Warren, Otis, Calkins, and Peterson, all of whom, except Otis and Calkins, are behaviorists with certain reservations.

And, were the writer asked to state briefly what he considers to be the most patent faults of ultra behaviorism, as represented in Watson's system, as well as in that of Doctor Perrin as it now stands, he would unhesitatingly make the following reply: (1) The psychological reader, as well as the lay reader, gets the impression that the human nervous system is a hodge-podge of reflex-arcs, or habit systems, which are not controlled in any regular or organized or methodical fashion. (2) The ultra behavioristic system is reared upon a concept of *integration*, but almost no attempt is made to tell one just where and just what the integration is, how it operates there, and how integrations are first effected there and later how they are dissembled and transformed there. In fact the writer believes that one can with pencil and pad go through Watson's second book, checking and counting the appearances of the word 'integration,' and the word 'pattern,' and that in so doing one will discover a great paucity of actual descriptions of just what integration really is or just what 'patterns' are except for statements concerning the complications and concatenations of peripheral stimuli, on one hand, and the complications and sequences of muscular and glandular acts, on the other hand, with an occasional reference to the summation of stimuli and to the final motor pathway. (3) The system lays so much stress upon peripheral stimuli, on one hand, and upon response by muscle and gland, on the other hand, that one gets the impression that nothing of consequence occurs in the central nervous system¹ itself. (4) In the Watsonian system, if it were

¹ In fact Watson says, *op. cit.*, pp. 149 and 154, that we have in the past made too much of the associative areas in the cerebrum 'and of the whole localization of function as well'—that the 'nervous system' should not 'be overemphasized'; and, by implication, that, in action and thought, etc., the function of the central nervous system is not greater than that of the heart, bones, glands, and muscles.

granted that all happenings of the central portions of arcs had their proportionate effects in acts and in ideation, no room is left for classifying those *central* integrations into higher and lower orders or into any qualitative orders whatsoever. And (5) the human being, so behavioristically treated, must become a mere weathervane, which is beat around incessantly by peripheral stimuli, regardless of whether these stimuli are extero-, intero-, or proprioceptive in nature. This mechanical automaton can have no power to control self, or to choose or select lines of action; nor can it make use of the stored and altered and organized residua of old impressions in the cortex and elsewhere in the cerebrum. We find ourselves quite unable to consider the human being such an automaton, and we consider that not a few of the arguments advanced in promotion of such a doctrine really beg the question, as Watson readily admits, concerning one of his assumptions.¹ We find ourselves in sympathy with the attempts of Warren to meet certain seriously objectionable features of behaviorism, but we feel also that Warren can neither prove that consciousness *is*, that is, *is identical with*, nerve impulse nor distinguish between those excitations (in reflexes, etc.), which are not related to consciousness and those which are linked up somehow with consciousness. Parenthetically, we add, we believe that certain nerve impulses are consciousness or are responsible for consciousness, but we realize fully our inability to prove logically that nerve impulse and consciousness are identical. We estimate more highly than do some of the behaviorists the philosophical and epistemological problems here involved, and we believe that nothing less than presumptuousness will characterize him who in arbitrary fashion kicks these problems out of the psychological backdoor.

We note also that Perrin is inclined to incorporate in the answers which he has given to his own queries the behavioristic concepts of 'implicit' and 'overt' motor reactions. We

¹*Op. cit.*, p. 326. We submit that this assumption should have been clearly stated in bold type in both Chapters I and II, where Watson discusses the problems, the scope, and the methods of psychology!

must frankly confess our own obtuseness in getting meaning out of these two terms 'implicit' and 'explicit' or 'overt'; we should like to receive a full, definite, and clear definition of each of them. As currently used by behaviorists, 'explicit' or 'overt' refers to the actual muscular movement itself, perhaps in a form discernible to another person; and 'implicit' is used to cover visual images, auditory images, and all other kinds of images, and, in some cases, the matter is carried to such lengths as to deny the actuality of all images except kinaesthetic images and these are presumed to be not images but sensations of actually innervated muscular movements of a slight and not so observable extent. The writer craves light upon the presumed fact that a muscular movement is the 'explicit' characteristic of an act and a visual image is the 'implicit' counterpart of that very same 'explicit' act.

In conclusion, we hold no brief for consciousness as consciousness, for consciousness as a set of spiritual or psychical or magical causative agencies. We believe that a great many of the investigators who esteem highly the introspective method are in no sense interested in consciousness *qua* psychical forces and spiritual agencies and doers of magic. However, we do believe that conscious processes are criteria for the presence of certain cerebral and cortical integrations and the workings of integrated mechanisms, and that, frequently, these integrations and these mechanisms can be known in no other fashion.¹ We believe that it will be a long, long while before, by other means, we shall obtain very much physiological and biochemical knowledge concerning mechanisms and integrations and disintegrations which operate in the cerebrum and the cortex; and that, during this long, long

¹ The suspicion has haunted us for a long time that both Yerkes and Watson, in their distrust of the efficacy of the introspective method, have been unable in their articles and books to avoid the using of results and hypotheses which had earlier really been fashioned out through introspection. We refer especially to the hypothesis of Yerkes, that great variability, inconstancy, and unpredictability of an animal's responses indicate the employment of images and ideas by that animal; and to the hypothesis of Watson that thought consists solely of 'inner speech.' We believe that an historical account of these two concepts will clearly show that neither Yerkes nor Watson has been able to discard entirely the results which have accrued from introspection.

wait, there will be no lack of effort and zeal to persuade us that one is at the mercy of one's peripheral stimuli and of one's muscles and glands,—that one has no control by cerebral and cortical means of one's acts,—and that cortical organizations of the effects of past stimulations do not exist or, if they exist, they are impotent. We would point out that, in Watson's first book,¹ the selection and fixation of the correct acts in learning were effected through means of *frequency* and *recency*; and that, in Watson's second book,² *frequency* and *recency* are supplemented by (1) the general setting of the situation, (2) the situations which have just preceded, (3) intra-organic factors (like toothache, headache, etc.), and (4) the attitudes which have been developed in the individual by his life history and his past experiences. Are these significant additions harbingers of other additions which will be given to us in the course of time in his later books, so that, ultimately, his list will be as large as, and very similar to, the long list by Meumann³ and others?

¹ 'Behavior—An Introduction to Comparative Psychology,' N. Y., Henry Holt & Co., 1914, p. 256 ff.

² *Op. cit.*, p. 298 ff.

³ 'The Psychology of Learning,' D. Appleton & Co., N. Y., 1913.